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(a-c)

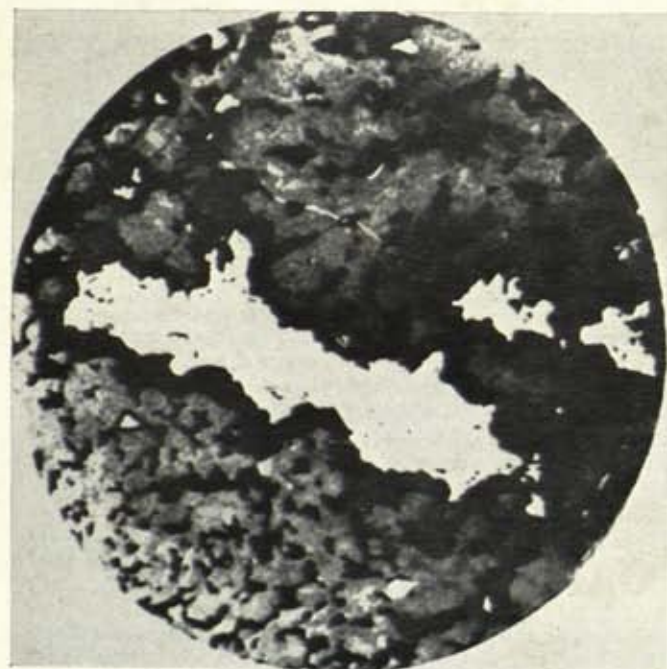


(d)

### REMARKABLE NEW FINDS AT IFE, WESTERN NIGERIA

As this issue goes to press, information has reached the Hon. Editor of MAN from Mr. Bernard Fagg, Director of Antiquities, Nigeria, of a most important discovery of ancient works of art in bronze or brass, made at the end of November by a builder's workman levelling a low mound at Ita Yemoo on the outskirts of the town of Ife. Archaeological excavation of the site was immediately arranged for. The above photographs show some of the bronzes, before cleaning: (a) a figure, 19 inches high, of an Oni of Ife in the full regalia still used at coronation, in his left hand the horn of authority, in his right a sceptre (said nowadays to be of white cloth and coral beads); (b) a pair of royal figures arm-in-arm (damaged by workman's pick); (c) a 'small but fairly heavy' casting in the form of a pedestal bowl with a royal figure encircling the bowl, which is supported on a circular ritual stool similar to the well-known quartz specimen in the British Museum; (d) two brass human-headed staffs (with a pottery object behind). There are also two egg-shaped objects each with two naturalistic heads attached. Comparison may be made with the Ife bronzes reproduced in MAN, 1949, 1, or in Underwood's *Bronzes of West Africa* (London, 1949). The following provisional comments are offered by William Fagg, Deputy Keeper, Department of Ethnography, British Museum: (1) The most surprising feature of the figures (a, b) is that, unlike the naturalistic faces, they conform to 'African' proportion, just as in modern Yoruba wood carving, the heads being proportionately more than twice as large as in nature. (2) While (b) may be a royal couple, it is perhaps possible, if cleaning shows the legs to be intertwined as at present appears, that it depicts the ritual struggle of the king with another chief at his installation. (3) The bowl (c) confirms the evidence, provided by the fragmentary terra-cotta royal figure seated on a similar circular stool with projecting loop (found at the Iwinrin grove and now in the Ife Museum), that the British Museum and other stone carvings from Ife are contemporary with the bronzes and terra-cottas. (4) The 'sceptre' in (a) in the Ife Museum), that the British Museum and other stone carvings from Ife are contemporary with the bronzes and terra-cottas. (5) The beardlessness of the figures tends to support the view that the rows of holes found on most of the bronze heads were not for the attachment of a beard, but rather some such ritual band as that seen on the nearer staff in (d). Photographs: Bernard Fagg, 1957





# THE EARLY METALLURGY OF COPPER AND BRONZE

(a) Twinned structure, characteristic of gold, copper, silver, etc., when worked and annealed or hot-worked; (b) Cored structure of copper in as-cast state; (c) Galena (black) containing nickel arsenide (grey) and quartz inclusions (white) (After Trillat and Legrand); (d) Slip bands in cold-worked copper



# THE EARLY METALLURGY OF COPPER AND BRONZE\*

A REPORT TO THE ANCIENT MINING AND METALLURGY COMMITTEE  
OF THE ROYAL ANTHROPOLOGICAL INSTITUTE,

by

F. C. THOMPSON

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## Native Metals

I It is generally agreed that the first metals known to man were those which occur native and, where these occur, the earliest would be the nuggets of gold to be seen in the gravels of river beds.

That such native metals could be formed into different shapes by hammering would be a discovery easy to make. Such hammering, however, hardens the metal and renders it brittle, and a time will come, therefore, when further working results in the development of cracks. To inhibit this it must be annealed, *i.e.* heated to a temperature which, in the case of gold or the very pure native copper, is far below a red heat. The discovery of this simple fact cannot have been long delayed. To throw a broken piece into a fire in a fit of temper is an almost instinctive act: to attempt later to salvage a part is equally natural.

At a later stage the discovery that such material could be melted would follow. There has, I believe, been some discussion regarding the order in which worked and cast implements were first produced. In the mind of a metallurgist there should, I think, be little doubt that casting was the later process when one considers the relatively advanced technique involved in the melting of the metal, its transference in some appropriate container to the mould, the preparation of this mould and the pouring of the liquid metal under such conditions that a sound casting free from gas holes is obtained. The soundness of the castings in copper—not too easy a metal to deal with—produced by the earliest metallurgists is proof of their skill.

Copper occurs in the metallic state in many parts of the world, sometimes in considerable masses, and as the analysis I in Table I shows, normally in a state of a high degree of purity. It is, therefore, soft and easily worked, as a result of which it can be hardened considerably, a fair cutting edge being thus obtained. This material then, there is every reason to believe, would be the first to be applied to utilitarian purposes.

There appears, in certain quarters at any rate, to be the belief that it is possible to identify such native metals from their micro-structures. Reference<sup>1</sup> has been made to the 'peculiar' twinned structure of hammered native copper (Plate Ba), or the cored structure of such native copper when remelted and cast (Plate Bb). It cannot be too strongly emphasized that neither of these structures is in any way characteristic of native metal as such; they are common not only to ordinary metals such as gold, silver or copper, but also to a whole range of man-made alloys; it is the treatment which gives rise to these structures which

TABLE I. COPPER

	I	II	III	IV	V
	Native Copper	Early Third Millennium (Malachite?)	Middle Third Millennium (Malachite?)	Later Third Millennium (Weathered pyritic ore?)	Millennium (Pyritic ore?)
Copper	(99.96)	(99.43)	(99.7)	(98.98)	97.41
Tin	0.0005	0.02	0.03	0.5	0.87
Iron	0.0002	0.3	—	0.1	0.02
Nickel	0.03	0.005	0.002	0.03	0.16
Sulphur	—	—	—	—	—
Lead	n.d.	0.002	0.01	0.03	0.56
Arsenic	n.d.	0.1	0.2	0.3	0.20
Zinc	n.d.	0.005	0.005	—	—
Bismuth	n.d.	0.0005	0.002	0.0006	—
Antimony	n.d.	0.03	0.03	0.05	—
Silver	0.0025	0.1	0.01	0.01	—

*n.d.* = not detected spectrographically. Figures for copper in brackets are by difference. References: I, MAN, 1948, 17; II, Burton-Brown (see note 6), p. 189; III, *ibid.*, p. 191; IV, *ibid.*, p. 193; V, *ibid.*, p. 193.

are found alike in native metals and in artificial materials of similar composition.

There does seem some possibility that native copper, so long as it has not been remelted, can be identified by the complete absence of oxide inclusions, but even here it would be nice to have further information regarding the possibility of the existence of other inclusions such as sulphides which, when small, are most difficult to distinguish from the oxide itself. The identification, then, of the copper as being native is a matter of no small difficulty which can probably not always be done with complete certainty and in any case requires careful investigation by an experienced metallographer. The matter is complicated still further by the fact that it is possible, by fire refining and subsequent deoxidation by poling, to produce copper of a purity at least equal to that of the normal native metal.

There is still one further point in relation to the micro-structure of native metals to which brief reference may be made. If a large gold nugget dredged from a stream is sectioned, polished and etched in aqua regia, as Liversage showed as far back as 1895,<sup>2</sup> a twinned structure similar to that of Plate Ba is obtained. This may be interpreted in terms of modern metallographic knowledge as showing that this mass of metal started as a dust in the parent rock, the individual grains of gold being subsequently welded into bigger and bigger pieces under the hammering of the river gravel. In this cold-worked state the grains are at first deformed and elongated but the pure metal will, even at ordinary temperatures, recrystallize in the course of millennia, thus becoming soft again and with the twinned structure of Plate Ba.

In the case of massive metal present in the original rock,

\* With Plate B, four text figures and three tables



from which it presumably separated whilst both were molten, such hammering necessary for the production of the twinned structure has not occurred and a different type of structure is obtained. A word of warning, however, is required. When metals deform they do so by a process of slip in which whole slabs glide over each other much as cards slip over each other in a pack. Such slipping results in the formation of characteristic lines, 'slip bands,' across the crystals of which the metal is composed (Plate B*d*), and hence affords evidence of the deformation to which the latter has been subjected.

Such slip bands have been observed in samples of native copper and ascribed to deformation due to earth movements. Whilst such movements could cause this type of structure, it cannot be stressed too strongly that it may also result from the very act of extraction of the sample of the metal from its matrix unless most extreme care is taken throughout the whole of the process.

#### *Extraction from the Ore*

The pure ores of copper, as a result of their brilliant and attractive colour, must have attracted the attention of early man wherever they cropped out at the surface. Exactly how it was first discovered that certain of such ores could be reduced by heating them with charcoal is a matter of mere speculation; whether it was the domestic fire or a pottery kiln, or, more fancifully, a forest fire started by lightning flash, is immaterial; in some way or other the reduction did occur and the mind of an observer was induced to correlate the ore with the treatment and with the product.

Where the outcrop ore was relatively soft, as is normally the case where weathering has occurred, the ordinary mining technique of the times with antler picks and wooden shovels would be sufficient. In the case of harder ores, however, these were certainly mined by 'fire-setting,' in which a fire was built against the ore face, which when sufficiently hot was quenched with water, splitting off fragments of the rock. Figs. 1 and 2 illustrate this process, the first representing this method of mining around 1500 B.C., the second taken from Agricola's *De Re Metallica*, of the middle of the sixteenth century A.D. The remarkable persistence of this technique—which could be duplicated in many other instances—is of value as justifying the use of information obtained in comparatively recent times in relation to procedures in those long past.

Ore bodies, however, are not normally composed of the pure minerals; the ore itself may be admixed with other ores and with 'gangue' or waste matter, and as a preliminary to the smelting process itself some sort of purification is generally required. Such processes in very early days could consist of handpicking, crushing and washing in a stream of water to remove clay or sand, and there can be little doubt that early man would soon see the advantages of these.

Such ore-dressing, for instance, was employed by the native Bavenda and is described by Stayt as follows: 'The ore . . . was broken up and cobbled down to eliminate



FIG. 1. RECONSTRUCTION OF A COPPER MINE AT MITTERBERG IN THE TIROL

Fire-setting is being used to break up the rock face; c. 1600–1800 B.C. By permission of the Clarendon Press, Oxford, from *History of Technology*, Vol. I, after Hahne, *Bergbau in der Vorzeit*, Vol. II



FIG. 2. FIRE-SETTING

A, Kindled logs; B, sticks shaved down fan-shaped; C, tunnel. After Agricola



waste rock, making thereby a rough concentrate which was ultimately carried to the smelting furnaces. Large heaps of this broken waste have been found around the excavations.

In the case of carbonate ores, such as malachite, the concentrate is now ready for reduction to the metal, a relatively simple process involving nothing more than heating with charcoal to a temperature around 600°C., a very dull red heat.

TABLE II

Metal	Melting Point (when pure) °C.	Heat required to reduce 1 gm. of metal from oxide, K. Cals.	Temperature at which reduction commences, °C.
Copper	1083	0.11	500-600*
Lead	327	0.125	500-600*
Iron	1537	0.68	c. 900
Tin	232	0.73	1000-1100*

\* Gowland

In Table II are collected certain metallurgical data regarding the properties of copper, lead, iron and tin, from which it will be seen that (apart from the actual melting) copper is as easy to reduce to the metallic state as is lead, whilst iron and tin are both distinctly more difficult. It is not surprising, therefore, that copper was the first of these metals to be produced by man. By hand-picking specially pure pieces of the ore reasonably pure samples of copper—though less pure than the best of the native coppers—can be produced (analyses II and III of Table I).

The product of the reduction at this low temperature is, however, a mere spongy mass of metallic copper together with impurities from the ore and unburned charcoal. Such an aggregate is not easy to reduce to a coherent form by hammering, but it can be done and it must be remembered that the low-temperature reduction of an iron ore leads to an exactly similar 'sponge,' and that for millennia this was the only way in which iron or, later, steel, could be produced, by successively heating and hammering the at first quite friable mass. If, however, a sufficiently high temperature can be developed it is clearly very much easier to effect this coherence by fusion, when the molten copper will collect at the bottom of the furnace under a scum of unfused impurities. The temperature required for this—1083°C.—is, however, very much higher than that needed for reduction, approaching a yellow heat, and some sort of blast, either natural or artificially produced, would normally be required. The discovery that the addition of suitable fluxes would convert this scum to a liquid slag floating on the metal paved the way for all subsequent developments. In this way the cakes of copper, such as that shown in fig. 3, were produced.

It is no real function of mine to speculate regarding the manner in which primitive man was led to his discoveries, but as a metallurgist there is one point which I may legitimately raise regarding the way in which the first smelting of copper ore took place. It has been said<sup>4</sup> that the camp fire reaches a temperature too low for the reduc-

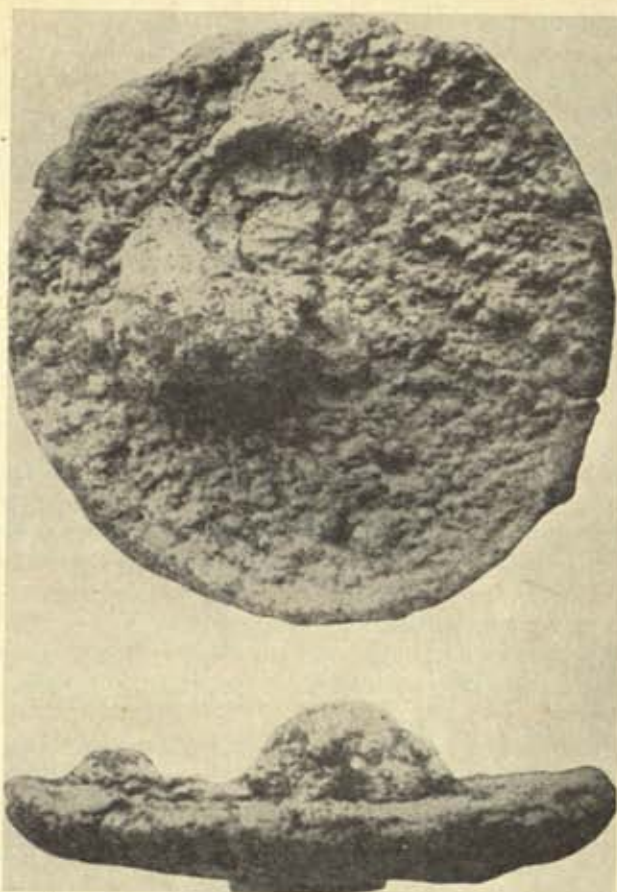


FIG. 3. ROMAN CAKE OF COPPER FROM AMLWCH, ANGLESEY

Diameter: 11½ inches. By courtesy of the Trustees of the British Museum

tion of copper oxide with carbon, the maximum temperature of the fire being put at 600°C. to 700°C. and the minimum temperature of reduction being stated as 700°C. to 800°C. Both of these observations seem to me to be in error. A temperature of 600°C. is so low that the fire would hardly be visible in strong sunlight, whilst, as the free-energy diagram shows, the temperature at which copper oxide can be reduced by carbon is well below 600°C. There is nothing that I know which precludes the possibility of the first reduction of a copper carbonate ore having occurred in a domestic fire drawn up by a hot, dry wind.

### Sulphide Ores

Where ore bodies crop out at the surface as a result of weathering they tend to become oxidized and to disintegrate. As the depth of mining increases, therefore, they not only tend to become harder but may also change radically in composition. A fundamental step in the whole history of civilization was taken when some early genius realized that copper also occurred in the unweathered sulphide ores. It may well have been that the golden colour of pyrites misled him into the belief that he was extracting



gold, but however this may have been, sooner or later it was realized that such ores did contain copper and processes involving relatively very advanced metallurgical knowledge were gradually devised. It can hardly be doubted that the men responsible for this outstanding achievement were actuated by a real curiosity regarding natural phenomena and were, in the best sense of the word, the research workers of their day.

(a) *Roasting*. Since cuprous sulphide, unlike the oxide, cannot be reduced to metal by heating it with carbon, the first stage in the extraction of copper from such an ore is the subjection of the concentrated material to a roasting process, *i.e.* heating to a relatively low temperature, but above a red heat, with free access of air. As a result, quite apart from physical effects such as the breaking-up of the larger lumps, a partial or complete oxidation occurs, with the partial or complete removal of elements such as sulphur, arsenic or antimony which form volatile oxides.

When dealing with such ores, the process in its simplest form consists of nothing more than building a platform of fuel, possibly with vertical pillars, and piling the ore in a heap on and around these. The ignition of the fuel starts the process which, owing to the heat of oxidation of the sulphur, is then self-continuing, the combustion needing, however, vents and chimneys through which the air can percolate into the centre of the mass and the gaseous products of reaction can escape. Simple as this sounds, the reactions involved may be highly complex, and to carry it out efficiently requires no inconsiderable degree of skill. It may continue for days, during which time constant vigilance is required to ensure that an adequate temperature is maintained, but that this does not become too high. In the latter event fritting, *i.e.* partial fusion, may occur as a result of which the roasting reactions are brought more or less to a standstill.

Fig. 4, taken from Agricola, illustrates what must have occurred some 4,000 years earlier.

In addition to copper, iron and sulphur, pyritic ores often contain not inappreciable quantities of arsenic, antimony, zinc, nickel, etc., and of these varying proportions, depending on the exact manner in which the process is carried out, will ultimately find their way into the metal.

Where arsenic and antimony are present, as is normally the case with such ores, special conditions obtain. The oxidation of these elements to  $As_2O_3$  and  $Sb_2O_3$ , respectively, results in these volatile compounds being carried away in the fumes from the heap. A part, however, is converted into the higher oxides  $As_2O_5$  and  $Sb_2O_5$ , which combine with the metallic oxides to form much more stable arsenates and antimonates. These being non-volatile at the ordinary roasting temperatures are subsequently reduced with, and pass into, the copper. The proportion of these elements present in the ore which ultimately enters the metal depends, therefore, on the exact conditions under which the roasting is carried out. It will depend on such factors as the size of the pieces of ore, the way these are packed, the air supply, the time and the temperature reached, *i.e.* how well the lumps of ore are broken



FIG. 4. ROASTING OF COPPER ORE

A, Heap of cupriferous stones; B, kindled heap; C, stones being taken to the beds of faggots. After Agricola

down, how loosely the heap is packed, and whether the wind is strong.

There is, therefore, no direct correlation between the content of these impurities in the ore, which, it must not be forgotten, may itself vary considerably even over distances of only a few feet, and in the metal; and at any given mine the composition of the roast and hence of the metal produced may have varied almost from day to day. Further, even when a crystal of the apparently pure mineral has been picked out, it must not be forgotten that it may still contain inclusions of other substances. This is well illustrated in the microradiograph of a crystal of galena (Plate Bc), which contains inclusions both of quartz and of nickel arsenide. Such inclusions are by no means uniformly distributed and may vary to a considerable degree in samples of ore taken within inches of each other. Far too much weight, then, has been given by some archaeologists to variations in the analyses of their finds; variations which are often no more than are to be expected from the normal metallurgical practice of the day. Winter or summer, wet weather or dry, calm or windy weather, may all lead to considerable variations in the metal produced from a given ore. It would indeed be satisfactory if we could correlate the exact location of the ore with the analysis of the copper produced, but this can be done only in the roughest of ways. This must not be taken to imply that the work being done in analysing metallic samples and ores is without real value. What is required is a more critical consideration of such results by those well versed in metallurgical knowledge.

(b) *Smelting*. The extraction of the metal from pure cuprous sulphide ores presents no difficulty. All that is required is a 'dead roast,' *i.e.* the complete elimination of the sulphur and the conversion of the sulphide to the oxide, followed by reduction as before with charcoal.



In the case of copper pyrites, however, a mixed sulphide of copper and iron, the operation is far more complex.

As a result of the roasting the ore has been partially desulphurized and partly oxidized, and consists, at any rate for our purpose, of the sulphides of copper and iron together with oxide of iron in varying proportions depending on the exact nature of the roasting process. The extraction of the copper in a state of reasonable purity from this roast presents far greater difficulties than obtain with the reduction of carbonate or the pure sulphide ores since the amount of iron present normally far exceeds that of the copper, and it is the removal of this iron which is the real problem.

The basis of the separation of these metals depends essentially on two chemical facts, first that where there is insufficient sulphur to combine with both metals this element attaches itself preferentially to the copper, and secondly that in the case of oxygen the reverse is true, so that, on melting, the roasted ore tends to yield a mixture of sulphide of copper and oxide of iron. If sand is added to the molten charge, this combines with the oxide of iron to form ferrous silicate as a slag which, being lighter, floats on the surface of the molten copper sulphide. On solidification the slag can be broken away from the latter and, if purification is not complete, as would in all probability be the case in primitive times, the roasting and melting processes may be repeated again and again until practically pure cuprous sulphide is obtained. This 'matte,' i.e. the copper sulphide, when sufficiently freed from iron, may then be dead-roasted, converted into the oxide and then reduced to the metal with charcoal. There is nothing in this sequence of operations which makes any special demand on equipment, the temperatures required are no higher than that needed to melt native copper, and there can be little doubt that it was in this way—as, in the broadest outlines, it still is—that the copper was extracted.

Given, then, a body of men sufficiently curious to experiment for themselves—and of this, so far as I can see, there can be little doubt—the production of copper from pyritic ores, though by no means an easy problem, presents no insuperable difficulty, and as the supply of the carbonate ores, which are far less common than the pyritic, together with the relatively easily reducible oxidized ores due to surface weathering of sulphide ores was used up, early man was, in fact, driven to the discovery of techniques by which pyrites was compelled to yield up its metal.

That the problem of the elimination of the iron—that of the sulphur presents no difficulty—was not solved quickly is only to be expected and there is certain evidence of early, unsuccessful attempts to produce a copper relatively free from iron. Forbes<sup>5</sup> reports that a piece of matte found at Ugarit contained 35 per cent. of copper, and the so-called blue metal 84.1–87.2 per cent. of copper, 12.7–14.1 per cent. of iron and 0.2–0.5 per cent. of sulphur, whilst the metal contained 98 per cent. of copper and traces of iron, tin, lead, zinc and sulphur (0.3 per cent.). The composition of the matte may be regarded as reasonably

normal but that of the 'blue metal' with some 13 per cent. of iron is far from this.

It is not impossible, however, that the figures for sulphur and iron have, in fact, been transposed, in which case the composition of the 'blue metal,' i.e. the cuprous sulphide, corresponds reasonably well with that of the corresponding modern product. If this be so, the technique for the extraction of copper from pyritic ores had already been mastered. The probability of such a transposition is supported by another analysis<sup>6</sup> of a sample from Troy (about 2,300 B.C.) which contained 79.66 per cent. of copper and 0.08 per cent. of iron with 19.5 per cent. of sulphur, almost the theoretical composition of cuprous sulphide.

Two other analyses of interest in this connexion are given by Burton-Brown, who reports a sample of copper of the late third millennium B.C. of the composition 93 per cent. of copper, 5.9 per cent. of iron and 1.0 per cent. of sulphur. Here again, the removal of the iron has been far from complete. Another sample, a bronze of the second millennium B.C. containing 16 per cent. of tin, although still containing 0.6 per cent. of sulphur, had only 0.1 per cent. of iron. In this case it would appear that the last element has been successfully removed, though the final smelting of the cuprous sulphide matte has been incomplete.

It can hardly be doubted that the examples cited do refer to sulphide ores and that quite successful attempts were being made to treat these materials not later at any rate than the end of the third millennium B.C.

Let us now consider the fate of the impurities in the ore which were not removed, or were removed only in part, by the roasting process. During the melting for the production of the matte we have, as has been said, two separate liquids, the heavier matte below and, floating on it, the lighter slag. Between these two immiscible layers there will be a partition of the impurities. Since, however, there will be a variation in the composition of the slag according to the degree of oxidation of the iron sulphide during the roast, and also according to the amount of silica added, and since the partition coefficient itself varies with temperature which will not be constant in these primitive operations, the amount of the impurities which passes from matte to slag will be a variable depending on the exact conditions of each melt. It is not easy to suggest how much of the impurities is lost in the slag, but some purification will occur, increasing with the number of times the operation is repeated; but it will never have been complete, and will have varied from element to element.

As the early metallurgists were driven to the use of more and more impure ores, therefore, the purity of the copper produced progressively deteriorated. This may have reached a climax with the use of the *Fahlerz* of Central Europe, a cuprous sulphide ore in which part of the copper is replaced by varying amounts of iron, zinc, silver and mercury and which is further associated with changing proportions of the sulphides of arsenic and antimony. It is my belief that it was due to ores of this kind that the group of impure coppers, illustrated by analyses IV and V



TABLE III. BRONZE

SPECIAL 'BRONZES'					TIN BRONZE				
A	B	C	D	VI	VII	VIII	IX		
Later Third Millennium				Later Third Millennium		Second Millennium	Composition		
(Arsenic)	(Lead)	(Iron)	(Arsenic) (Refined Copper?)	(Impure Copper)		(Refined Copper)	of Copper calculated from VIII		
Copper . . .	95.0	85.8	93.0	(~ 92.8)	(93.84)	86.93	(99.4)		
Tin . . . .	Tr.	3.5	—	n.d.	5.0	10.62	?		
Iron . . . .	0.3	0.2	5.9	< 0.01	0.5	0.25	0.11		
Nickel . . .	0.1	—	—	0.1	0.05	0.49	0.33		
Sulphur . .	—	—	1.0	—	—	—	—		
Lead . . . .	0.3	8.5	—	0.01	0.2	0.68	0.033		
Arsenic . . .	4.2	—	0.1	~ 7	0.2	0.77	0.055		
Zinc . . . .	—	—	—	n.d.	—	—	—		
Bismuth . .	Tr.	—	—	0.02	0.002	—	0.0005		
Antimony .	—	—	—	0.05	0.2	0.32	0.033		
Silver . . .	—	—	—	0.02	0.005	—	0.011		

Tr.=trace. n.d.=not detected spectrographically. Figures for copper in brackets are by difference. References: A-C, Burton-Brown (see note 6), p. 193; D, MAN, 1948, 17; VI, Burton-Brown, p. 193; VII, *ibid.*, p. 194; VIII, *ibid.*, p. 196

in Table I, originated. Although it has been suggested that the impurities were deliberately added in order to harden the copper, it is inconceivable to me that such a complex alloy would, or could, be produced deliberately by the metallurgist of the times which we are considering. I am not unaware of the statement by Gowland that arsenic and antimony in corresponding amounts were deliberately added to the bronzes made in Japan for ornamental purposes; but the explanation given demands a knowledge of metallurgy which, so it seems to me, is difficult to assume at these very early times.

#### Bronze

If this be so, the use of the term 'bronze' for such materials is to be most strongly deprecated; they are 'impure coppers' and nothing more.

The influence of these impurities on the properties of the copper is twofold. In the first place they have a hardening effect, whilst secondly there is a very distinct improvement in the ease with which sound castings can be produced. As this improvement in quality became associated with the kind of ore used, the first step was taken towards mixing different ores to obtain a still better product. At first there can have been no appreciation of the reason for the increased hardness and 'castability,' and the earliest alloys deliberately produced must have had compositions determined by the kind of ores which were locally available. In this way the curious copper-arsenic and copper-antimony alloys may well have been produced. Some typical analyses, A, B, C and D, are shown in Table III and there seems every reason to believe that they were the result of definite intent; a content of arsenic, for instance, of around 7 per cent., with other impurities all below 0.05 per cent., can only point to the deliberate addition of a highly arsenical ore.

The replacement of arsenic and antimony by tin can hardly be explained except on the assumption of definite

experimentation. The brittleness of the high arsenical and antimonial alloys of copper, especially as chill castings, may well have been overestimated—an alloy with 8 per cent. antimony and 2 per cent. nickel, for instance, was suggested not so very long ago for gear wheels—but the true tin bronze is certainly an improvement on them. Starting with a tin content of 5 per cent. or less (VI), they settled down to bronzes with, normally, around 10–12 per cent. of tin, a composition clearly reached as a result of definite experimentation (VII) and consistent with modern metallurgical opinion.

*Refining of Copper.* If the published analyses of the true tin bronzes are examined they would appear to fall into two fairly distinct groups. In the first group the other impurities are present in amounts corresponding with those of our 'impure coppers,' analyses VI and VII, whilst in the second the impurities are as low as in the early coppers produced from pure, hand-picked malachite, analysis VIII. There appears, then, to have been a change, and for the better, in the kind of copper used about the first half of the second millennium B.C. About this time, too, I understand, there is evidence of the extraction of a crude copper at the mines which was then sent elsewhere to be refined.

The removal of the impurities in this crude metal must have been effected by the process now known as 'fire refining.' In this process the crude copper is melted on a saucer-shaped hearth in an oxidizing atmosphere assisted, if necessary, by further oxidation from an air blast projected on to the surface. Along with some of the copper, the more oxidizable impurities are removed as a scum and by continuing or repeating the operation, copper of a very high degree of purity can be produced—99.95 per cent. copper is commonly obtained in modern practice with occasional heats of over 99.98 per cent. As a result of the removal of the impurities the copper is rendered less brittle and, therefore, more amenable to working operations.



In such refining there is, however, the danger of over-oxidizing the copper and, therefore, of rendering it brittle once more. This over-oxidation can be removed by 'poling,' in which the molten, oxidized copper is vigorously stirred with green saplings, reducing gases from which react with, and reduce, the copper oxide. It would be a matter of very great interest to know at what stage in the history of metallurgy this latter fact was discovered, since it is a means of producing a metal almost as pure as that of native copper itself, even from ores which themselves were highly impure. It is significant that, apart of course from the tin, many of the later of these bronzes are of high purity, suggesting that the refining process was by then generally known and effectively carried out. Analyses VII and VIII illustrate the difference between an early and a late bronze of approximately the same content of tin, whilst in IX an attempt has been made to calculate the composition of the refined copper which was used for the latter and which may be compared with the unrefined copper of sample V. This is not, however, evidence that the beneficial effects of 'poling' were known, since the elements present in the bronze, tin, arsenic or antimony, are all themselves deoxidants and would, therefore, remove the cuprous oxide from the fire-refined metal.

There is a most interesting corollary to this. If the argument is sound, it would appear to follow that such pure bronzes can only have been produced from already refined copper with the deliberate addition of a pure tin ore (or, less probably, with tin itself). The earlier bronzes, on the other hand, were presumably produced by the straightforward reduction of mixed ores of copper and tin.

#### Summary

I may then summarize these views as follows. Starting from native copper, the purity of which is almost invariably very high, early man would first turn to pure oxide or carbonate ores such as malachite which, easily reduced by charcoal, would lead to the first artificial product. Where the ore had been picked out with care, such a product would be of relatively high purity though less pure than the native metal itself. With the exhaustion of outcrop ores, he turned sooner or later to the deeper, unweathered, less pure and more difficultly reducible ores of the pyritic type. In almost all cases such ores contain considerable proportions of impurities, some of which would

find their way into the metal. As has been pointed out, however, this proportion is by no means constant and varies with the particular conditions, particularly the roasting, under which each batch of copper was produced. We thus pass into an era of impure coppers, very variable in composition, which, in my view, owe these variations of composition to unintentional and accidental factors; I do not believe that such impure coppers, often erroneously termed bronzes, were deliberately produced.

The purification of impure copper by fire refining followed, and as skill and experience increased, another stage of relatively pure copper was introduced. It would appear to me that this stage of development may have corresponded in time more or less to that of the deliberate production of the tin bronzes, since these seem to be divisible into two groups, one in which the copper contained all sorts of other impurities, the other in which, apart from the tin itself, the purity was relatively high. The latter alloys can only have been produced deliberately—a statement which seems to apply equally to the so-called bronzes containing considerable proportions of arsenic, antimony, etc.

The sequence of the stages of development which has been discussed must be regarded merely as a norm. There may well have been some degree of overlapping, and it by no means follows that all stages are necessarily to be found in every locality. For these developments at least one and a half millennia seem to have been required, and possibly more, and those responsible emerge as possessing patience and intelligence not dissimilar from those of the research worker of the present day.

In this discussion of the metallurgy of copper the considerable variations which may occur from day to day have been stressed, and the warning issued that it is not justifiable, except in a general way, to expect to correlate the composition of the ore with that of the final metallic product.

#### Notes

- <sup>1</sup> R. J. Forbes, *Metallurgy in Antiquity*, Leiden, 1950, p. 319.
- <sup>2</sup> A. Liverside, *J. Chem. Soc.*, 1895.
- <sup>3</sup> H. A. Stayt, *The Bavenda*, London (O.U.P.), 1931.
- <sup>4</sup> H. H. Coghlan, *Notes on the Prehistoric Metallurgy of Copper and Bronze in the Old World*, Pitt Rivers Museum Occ. Pap., No. 4. O.U.P., 1951, p. 21.
- <sup>5</sup> R. J. Forbes, *loc. cit.*, p. 347.
- <sup>6</sup> T. Burton-Brown, *Excavations in Azarbaijan*, 1948, London (Murray), 1951, p. 193.

## CROSS-COUSIN MARRIAGE AMONG THE GARO OF ASSAM\*

by

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### 1. Marriage Relation between two Localized Lineage Groups

2 Garo<sup>1</sup> practise matrilineal descent and matrilineal residence and divide themselves into two major exogamous matri-phratries (*chatchi*), Sangma and

\* With four text figures

Marak.<sup>2</sup> Each major matri-phratry is segmented into many sub-groups (*mahari*),<sup>3</sup> which are again dispersed into various local lineage groups (see fig. 4). Structurally, these two *chatchi* constitute matri-moieties, but in practice exogamous marriages take place between two particular local



lineage groups which together form moieties; one particular local lineage A of one of the *mahari* of Sangma receives husbands from another particular lineage B of one of the *mahari* of Marak. Each lineage group forms one unit with a common ancestress associated with a particular locality, such as a village or a cluster of villages.

This marriage arrangement is closely associated with their economic institutions. The Garo ideal type of moiety arrangement is one formed by two villages, each of which is inhabited by one dominant lineage group. Each village possesses a village land (*akin*) where all inhabitants have rights of cultivation. This village land, therefore, is at the disposal of males of the opposite moiety who come in by marriage. Villagers of village A have rights in the *akin* of village B through marriage and *vice versa*. This relation is well represented by the Garo word *akhim*. *Akhim* indicates the right of marriage, or right of inheritance of the male line. By the *akhim*, a man of village A marries a woman of village B, and *vice versa*. Traditionally husbands are exchanged between two particular local lineage groups. Village A calls village B 'our *ajiksa*' and *vice versa*. *Ajiksa* means 'place or village of husband.' Thus, two exogamous lineage groups have reciprocal relations through cross-cousin marriages. However, as I shall presently explain, it is really a classificatory cross-cousin marriage that is involved.

Usually a local lineage group is dispersed over a wider range than one village. Especially since the end of head-hunting warfare between villages, it became common for villages to split, forming new branch villages around the parent village, owing to increasing population. By this process some families are separated from the parent village and this new local group takes some part of the original village land, creating a new *nokma* (headman). This branching-off of villages can often be traced in a reconstruction of village history. For instance, the village Romgagiri, one of the interior villages where my field work was carried on, has 12 satellite villages within a radius of five miles. Each of these satellite villages has village land, separated from the parent village and economically independent, but the *nokma*'s wife always belongs to the same lineage of the parent village. Likewise the *nokma* is always a man of the same lineage as the *nokma* of the parent village. Thus satellite villages together form one social unit with the parent village, though they form independent economic units. A local lineage group usually covers a wider range than one village.

## II. The Principle of Garo Cross-Cousin Marriage and its Actual Working

Garo cross-cousin marriage, which is symmetrical on the basis of local lineage groups taken as a whole, is not symmetrical from the point of view of each local line.<sup>4</sup> The actual practice of cross-cousin marriage between individuals is regulated by relation through descent lines. The Garo cross-cousin marriage is matrilineal, as Dr. Leach has pointed out.<sup>5</sup> A man marries his mother's brother's daughter; his sister is forbidden to marry his wife's

brother; further, his brother is also forbidden to marry his wife's sister. The reciprocal first-cousin marriage is thus excluded and the Garo avoid having more than one conjugal relation between members of two particular descent lines. This rule differentiates Garo-type marriage from the Kariera type,<sup>6</sup> in which a man marries mother's brother's daughter who is sister to his own sister's husband. On the other hand the Garo marriage system may be fairly regarded as a matrilineal version of the Murngin system. As with the Murngin, the general rule of marriage is that of matrilineal cross-cousin marriage and this is coupled with a moiety system such that the mother's brother of a man's mother's brother is a close relative of Ego himself though not of the same descent line. The relationship between the Murngin system and the Garo system may be seen if the diagrams given in this paper (figs. 1 and 2) are compared with the figures given in Dr. Leach's paper (figs. 6 and 7).<sup>7</sup>

Garo marriage rules for individuals are closely associated with the inheritance system. One particular daughter whom parents and members of the lineage have chosen as the heiress succeeds to the mother's property. She is called *nokna*. The husband of the *nokna* is called *nokkrom*, who is the successor of his father-in-law. The *nokkrom* should be the *nokna*'s father's sister's son (actual or classificatory). The cross-cousin principle applies only to marriages between *nokna* and *nokkrom*.<sup>8</sup> Other members are free from this principle. They can choose anybody from the opposite moiety group,<sup>9</sup> but they should avoid marriages with members of the families to which their sister's husband or their brother's wife belongs. The rigid cross-cousin rule in the case of the *nokna*'s marriage involves the desire for the continuation of *nok* (combination of two local lines). The Garo word *nok* has several meanings: it means a house, also the members of the household, but above all it has the meaning of a combination of local lines, the women who come from the village where the *nok* exists and their husbands who come from a village in the opposite moiety. We may consider the *nok* as a social institution placed between two notions 'family' and 'lineage.' The *nok* at a given time may take the form of a family—parents and their children—or sometimes includes a married daughter (heiress) and her husband with their children. The wife's married sibs and married sons and daughters, except the heiress, are excluded from the *nok*, forming new *nok* or marrying into another *nok*. The conception of the *nok* is also defined in abstract form in the time continuum, including dead members, not only as a model of contemporary existence. Among the Garo, the smallest social unit is not always considered to be the nuclear family, but is rather the *nok*.<sup>10</sup>

The central core of the *nok* is two persons, the husband who is the authority (head) and the wife who is the possessor of property. The *nok* is maintained by the constant succession of these two offices through cross-cousin marriage. The male line from mother's brother to sister's son and the female line from mother to daughter are established through an actual as well as a classificatory relationship. Strictly speaking the *nok* involves local lines rather than descent lines. Gaps caused by death should be filled by



members who have rights to it through kinship. The actual working of the rule of succession in the *nok* is shown in fig. 1.

The network of the succession overlaps descent lines by combinations of selected members of each moiety under various circumstances. It should be understood that descent lines are not the fundamental lines of successions with cross-cousin marriages, though the descent defines the categories. The institutionalized pair of succession lines, the *nok*, are local lines, not descent lines. Mr. Bose<sup>11</sup> and Mr. Mukherjee<sup>12</sup> confused this point because they did not

distinguish between the notion of local line and that of descent line, according to Dr. Leach's distinction.<sup>13</sup> Mr. Mukherjee, who contradicted Mr. Bose's statement that the mother-in-law should necessarily be the mother's brother's wife, reports that out of 22 marriages between *nokkrom* and *noknadona*<sup>14</sup> in four cases only was the father's brother's son involved. But this is what we might expect for the requirement is that the *nokkrom* should come from the partner *nok* in the opposite moiety rather than that the *nokkrom* should be a particular relative. Consequently the *nokkrom* are more likely to be classificatory cross-cousins than actual cross-cousins.

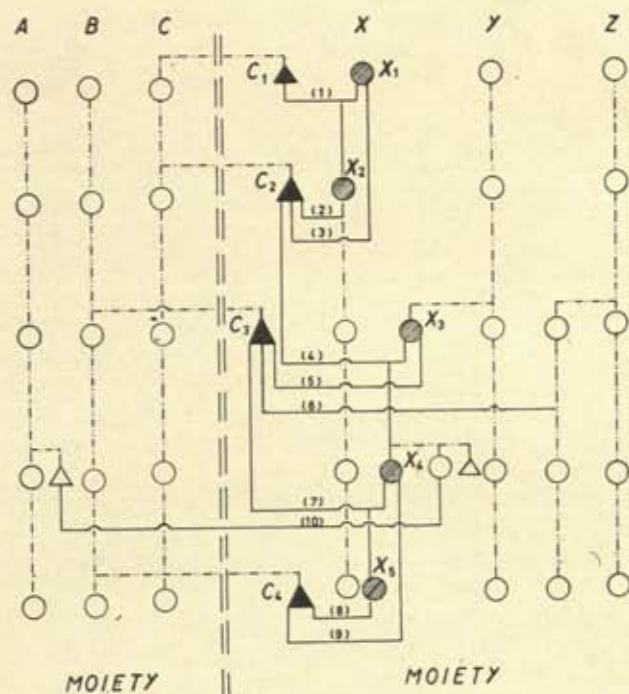


FIG. 1. EXAMPLES OF SUCCESSION IN GARO CROSS-COUSIN MARRIAGE

This diagram illustrates instances of successions of a *nok* from my field records of actual Garo cross-cousin marriages. Dotted lines indicate descent lines and continuous lines indicate local lines with marriage relations. A, B, C have one ancestress in common and so have X, Y, Z. (1) C1 married X1, as the ancestress of X1's father who is C1's brother. (2) C2 married X2, the heiress of X1, as the successor (*nokkrom*) of C1 (mother's brother). (3) After the death of C1, C2 married X1, according to the rule (X1 is the possessor of the property, not X2; to succeed C1, C2 should marry X1). After the death of X1, X2 succeeded to her place. (4) X2 died leaving C2. The place of X2 was taken by X3, because before X2 died, the daughter of X2 was not selected as the *nokna* (heiress) nor had C2 chosen his successor. (5) When C2 died leaving X3, all members of the moieties agreed to take C3 as C2's *nokkrom*. C2 did not have a direct sister's son, C3 married the widow X3 and X4, the daughter of C2 and X3 (7). (6) C3 was capable and becoming wealthy took one female of Z descent line as his third wife. (7) See (5) above. (8) C3 enjoyed long life. When his daughter X5 grew to marriageable age, he decided to allow one of his grand nephews, C4, to marry his daughter, *nokna* (heiress). (9) After some time he, C3, died and according to Garo rule C4 succeeded to C3's place by marrying his mother-in-law X4. (10) This is one of the instances of other members who are excluded from the succession of the *nok*. The sister of X4 married a man of A line, avoiding B and C lines. Likewise a brother of X4 has to marry a woman of the opposite moiety, who does not belong to A, B or C line; or he might be chosen as a *nokkrom* of some line of the opposite moiety, which coupled with the local line X.

### III. Nok and Local Descent Group

Garo cross-cousin marriage is practised between *nok*. The relation of husband-giving *nok* and husband-receiving *nok* is called *nok-chamé* by Garo. These relations are shown in fig. 2.

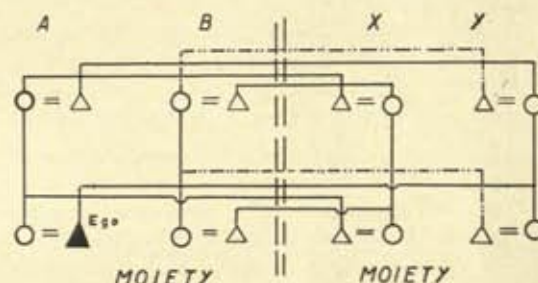


FIG. 2. 'NOK-CHAMÉ' RELATIONS

A, B and X, Y are four *nok* (local lines), extracted from each moiety which includes more than 50 *nok*. Dotted lines indicate one of the possible relations, and one also intended to show that the *nok-chamé* relations are not regularly circulating among *nok*: the husband-giving *nok* of B might be one of the *nok* of the opposite moiety, not Y. Though this diagram appears very similar to the Murngin system which was discussed by Dr. Leach (*loc. cit.*, p. 33), the delicate difference from the Murngin circulating marriage will be understood presently.

Every *nok* has two *nok-chamé* relations: *nok* to which the husband is given and *nok* from which the husband is received. These two *nok* should belong to the moiety opposite to that of Ego. In fig. 2, Ego was a member of *nok* Y before his marriage and by his marriage became a member of *nok* A. Thus a male member changes at his marriage to his mother's brother's *nok*. A female member, if she is the heiress, remains in the same *nok* after marriage by matrilineal matrilocal rule. However, a non-heiress (*agaté*) has to branch off from the parents' *nok*.<sup>15</sup> Except for the particular members who succeed in the *nok*, others are free to marry any partner whom they may find in the opposite moiety, provided that they do not marry members of a family into which their sisters or brothers have married. Actually all unmarried members are meant as a first reserve for the continuation of *nok*. The nature of the *nok* is such that a lack of members is expected. *Nok* A might fail to supply a husband to *nok* B, or *nok* B might fail to supply an heiress. In such an emergency the Garo look for members in the first reserve. The nearest in the



descent line is usually taken. However, it may depend also on liking or capability. The selection is made by a meeting of all members of both lineage groups of the husband and of the wife.

Though every *nok* is independent, the function of the *nok* is always backed by all people of a moiety. Thus any *nok* is prevented from developing social solidarity or differentiated status from the others. *Nokma*-ship (headmanship) also follows this principle. No doubt the *nokma*'s *nok* has prestige, being the oldest *nok* among *nok* of a village. The word *nokma* itself indicates 'mother of *nok*.' However, families of the *nokma* cannot claim superiority of status, because they are simply selected members from both local descent groups in which the members are equal in status to each other in each kinship network. Any male member of one lineage group may become *nokma*; the opportunity is provided according to circumstances. Usually the selection of *nokma*, when the late *nokma* has no direct sister's son, is accompanied by intrigues and competition among candidates.

This phenomenon is well understood because of lack of differentiation of status among *nok*. The relation of husband-giving and husband-receiving between particular *nok* cannot be permanent. The formation and continuation of *nok* depends on a whole network of local lineage groups which form a moiety group. The composition of the *nok* does not allow isolation of each lineage line differentiating from others, which develops in a system of circulating marriage relations, or economic or political compensation. Mr. Bose's statement<sup>16</sup> is wrong. He says:

'The *nokrom*, after the death of the headman, succeeds to the post and becomes the general manager of all lands of the village. On the other hand, a chowary,<sup>17</sup> who is sometimes the elder brother of the *nokrom*, has to live under his control and even has to pay tax to the headman as a member of the village, but never grudges his authority.'

There is no such custom of paying tax to the *nokma* (headman) among Garo. As I have said, every member of a village has the right to cultivate village land. The village land is divided into several parts on the basis of annual rotation of *jhum* (shifting) cultivation. Every year one of these parts is distributed to each household according to the number of its members. The share of the *nokma*'s family is allotted in the same way as the share of other families. The *nokma* has no special economic advantages over commoners. Sometimes indeed the *nokma*'s family is poorer than the others. The headman is the social, economic and political representative of a village, but he is not the controller as other tribal chiefs are. Usually he does not exercise authoritative power over villagers.<sup>18</sup> This is done by a village meeting.

Garo matrilineal cross-cousin marriage is thus differentiated from the Kachin type which results in different status among local lineage groups or a circulating marriage system in a society. We are here again reminded of the Australian Murngin system which manages to combine a matrilineal cross-cousin marriage system with status equality between the different local groups.

As in the Kariera type, Garo marriage relations are symmetrical on the basis of local lineage groups; but symmetrical relations or double marriage relations are not allowed between two families, which differentiates it from the Kariera type.

#### IV. Variation

My explanation of Garo cross-cousin marriage may appear too systematic. Those especially who develop theories from ethnographical reality, based on accurate field work, may think that this must be an ideal type of the Garo marriage system which is rather different in its actual working. But only in this way could I give the main orientation of the working of Garo cross-cousin marriage. In justification I can say that this type of marriage is dominant in their society.

Actual conditions at the present day are very complicated for two reasons: first, there is a minority practising patrilocal residence in a society whose arrangements are generally matrilineal; secondly, marriage relations sometimes extend to the third local lineage group which is differentiated from the opposite moiety.

The patrilocal residence brings complexity into the local arrangement of local lineage groups; in the locality of one lineage group A, there might be found women of lineage B, who belong to the opposite moiety to A. As I stated, non-heiresses and non-*nokrom* are free from the rigid cross-cousin principle. A man who is very much attached to his mother's village could bring his wife to his village, instead of going to his wife's village, which is the more usual arrangement. This wife, if she comes from the opposite moiety group, can have a house in her husband's village. However, she or her descendants are always addressed or classified as members of the opposite moiety. Her descendants are welcome under exogamous rules to marry any member of the village in which they were born. Thus marriage rules based on the lineage are more rigorously adhered to than marriage rules based on locality. So in practice the moiety group is not clearly marked by locality (see fig. 3).

The second variation on the traditional pattern also originated with those members of the family to whom the principle of Garo cross-cousin marriage does not apply. A

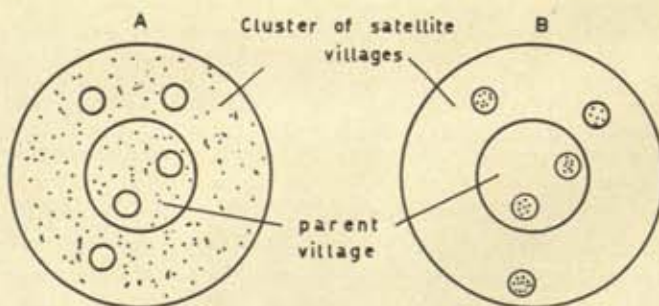


FIG. 3. VARIATIONS IN LOCALITY IN GARO MARRIAGE

A and B show the locality of lineages A and B. In the locality of A some households of lineage B (opposite moiety) are found and in the same way some of lineage A are in the locality of B.



man may sometimes marry a girl from the opposite moiety who is a member of quite a different local lineage group from that with which his own family ordinarily has relations. Through such a marriage a new *nok* is established. The logical succession rule of *nok* is applied to it. Thus a relation with a third local lineage group would be established, inserting itself into the traditional local moiety arrangement. The Garo marriage system involves by its nature the extension of marriage relation to a wider range. Possible cases of extension of marriage relation are shown in fig. 4.

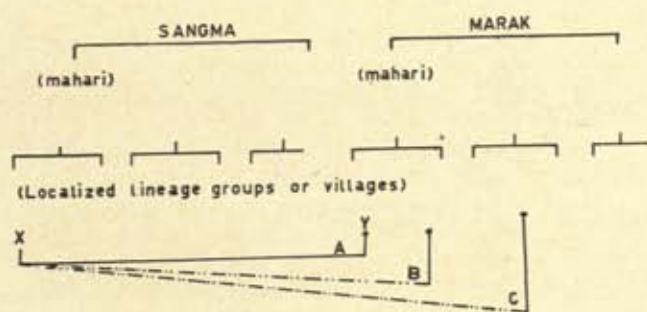


FIG. 4. VARIATION OF MARRIAGE RELATIONS

A, Traditional moiety arrangement (X-Y). B, Marriage relation with another local group of the same mahari with Y. C, Marriage relation with a local group of different mahari from Y.

However, the majority of Garo follow the traditional moiety arrangement, especially among older *nok*. In spite of the insertion of other elements into the social unit, the Garo do not allow the private ownership of village land so long as shifting cultivation is practised. Thus, village land is the clear indication of a traditional dominant local lineage group, though actually this includes various kinds of local lines in the course of time.

#### V. Economic and Political Aspects Correlated with Garo-Type Marriage

Shifting cultivation of the Garo type must be closely associated with the Garo-type marriage. When Garo start wet-rice cultivation, which makes private ownership of land essential, this disrupts the marriage system. Such economic change is a vital factor in the disintegration of matrilineal social networks, which in consequence tend to patriliney,<sup>19</sup> breaking the cross-cousin network.

The following economic propositions appear to be essential for the Garo-type marriage: (1) shifting cultivation, (2) land owned by a village, with no advantages given to any particular family or lineage, (3) shifting field with a permanent village site. I shall consider these three propositions with reference to tribes in Assam which I visited. The Khasi,<sup>20</sup> who lack (2), have quite a different social structure from the Garo, though they are matrilineal and matrilocal; they have no institutionalized cross-cousin marriage. Primitive tribes in Tripura State<sup>21</sup> lack (3); they shift the village after several years when the jungle round the village has been exhausted; they have no

institutionalized marriage regulation. The Lushai and Kuki are very similar to the Tripura tribes in their technique of shifting cultivation, lacking (3), and also (2); their system is the Kachin type. Thangkol Naga in Manipur State appear to fit all three of my hypothetical propositions of the Garo-type marriage institution; though they are patrilineal, exactly the same system is working among them as among the Garo.

According to Dr. Leach, a Kachin-type marriage system correlates with a political structure of a somewhat feudal type.<sup>22</sup> The Garo type appears to me to correlate with an equalitarian and democratic political structure. In fact, among Garo and Thangkol Naga, there are no chieftainships. A headman is only a nominal head, for important affairs are decided by village councils consisting of elders or heads of each family, including the headman.

#### Notes

<sup>1</sup> This paper is based on material gathered during 1955 and 1956 among matrilineal peoples in India; this field study was financed by a grant from the Elin Wägnér Foundation of Stockholm. The paper was written in England and I am especially grateful, for guidance in theoretical interpretations, to Professor Raymond Firth, University of London, and Dr. E. R. Leach, University of Cambridge. I am also grateful to Dr. Rodney Needham, University of Oxford, for detailed criticisms which improved the draft paper and to Dr. John Moge, University of Oxford, for encouragement and help in preparing the text for publication.

<sup>2</sup> The third major phratry, Momin, was created later and is a minor group. Momin can marry either Sangma or Marak.

<sup>3</sup> *mahari* corresponds to *machong* which was used by Playfair (*The Garo*, 1909). I take the term *mahari*, because this is used more frequently by Garo whom I had contact with, especially those who live in the central part of the Garo Hills where the marriage regulation is more rigorously functioning. Also I use *chatchi* in place of the *katchi* of Playfair.

<sup>4</sup> I follow Dr. Leach's definition on 'local line' differentiating from 'descent line'; see E. R. Leach, 'The Structural Implications of Matrilateral Cross-Cousin Marriage,' *J. R. Anthropol. Inst.*, Vol. LXXXI (1951), p. 25.

<sup>5</sup> E. R. Leach, *ibid.*, p. 25.

<sup>6</sup> Radcliffe-Brown, 'The Social Organization of Australian Tribes,' *Oceania*, Vol. I.

<sup>7</sup> E. R. Leach, *loc. cit.*

<sup>8</sup> Professor Lévi-Strauss called such a type of cross-cousin rule an 'exemplary line' in our talk at Paris, December, 1956.

<sup>9</sup> Since all the members of the opposite moiety belong to one's father's lineage, these marriages also become a cross-cousin relation in a broad sense. But a free choice of spouse is given to these members. As I shall explain later, they could even marry members outside the moiety.

<sup>10</sup> This notion of *nok* corresponds to such social institutions of various other societies, as *iye* of the Japanese, *tarowad* of the Nayar, *ing* of the Khasi and *familia* of the ancient Romans. In these societies, the nuclear family is not the actual social unit. Membership of these institutions is always clearly defined by a respective rule, which differentiates from the conception of extended family or of lineage.

<sup>11</sup> J. K. Bose, 'The Nokrom System of the Garo of Assam,' *MAN*, 1936, 37.

<sup>12</sup> Bhabananda Mukherjee, 'Mother-in-law Marriage among the Garos,' *Man in India*, Vol. XXXV, No. 4 (1955).

<sup>13</sup> E. R. Leach, *loc. cit.*, p. 25.

<sup>14</sup> I.e. *nokna* (heiress).

<sup>15</sup> Usually an *agaté* makes a new house near her parents' house within one or two years after her marriage. Thus new *nok* are created constantly by such members.



<sup>16</sup> J. K. Bose, *loc. cit.*

<sup>17</sup> *Chowary* means son-in-law in general, but Mr. Bose mistakes it for son-in-law excluding the *nokma*.

<sup>18</sup> Sometimes a very powerful *nokma* appears, and, being a capable man, manages village land as if it were his own private land, even buying other villages' land or selling his own village land. Such practices are against the rule. He forces the villagers to work in his field for nothing. However this is mainly because his superior personality takes advantage of being *nokma*. But this kind of power ceases in one generation because his successor usually lacks the same personality.

Another interesting Garo custom may be cited. A man may become very rich, thanks to his good management. This man is addressed as '*nokma-ganna*' after passing through a ceremony in which silver rings are put on his arms, after which he must make a great feast at his house. People who come to this feast are not only

his local lineage people but also people from various adjacent localities. They can stay at his house as long as they wish. During this time the *nokma-ganna* must give rice beer and meals for all people who come to his house. By such a fantastic feast, the *nokma-ganna* usually becomes the poorest man. He has to suffer in poverty by exchanging his wealth for his glorious title.

Both cases are simply a personal matter. The Garo social system does not institutionalize superior status or the economic advantage of a particular local line over others.

<sup>19</sup> I hope to take up this matter in another essay, 'Changes in Matrilineal Society.'

<sup>20</sup> I take the case of Khasi who practise *jhum* cultivation.

<sup>21</sup> Interrelation of their economy and social structure was studied in my essay 'Report on Primitive Tribes in Tripura State,' *J. Japanese Ethnol. Soc.*, Vol. XIX, No. 1 (1955).

<sup>22</sup> E. R. Leach, *loc. cit.*, p. 53.

## OBITUARY

### H. V. V. Noone: 1880-1955

3 In the death of Herbert Vander Vord Noone on 4 December, 1955, the Royal Anthropological Institute lost a devoted amateur whose primary interest was in primitive stone technology, and who made notable contributions in that field. His studies of the typology of stone-age artifacts benefited greatly from the fact that he experimented himself and continually had the tool-maker and his techniques in mind. Thus his studies of Upper Palaeolithic burins in France and on microlithic culture in Ceylon were entirely consonant with his eventual interest in the material culture and skills of the Australian aborigine.

H. V. V. Noone was born at Colombo, Ceylon, on 6 August, 1880, and at the age of six with his brother and sister he was brought to England by his mother for early schooling. At 13 he went out to India and at 18 years he began work there with a firm of export agents. This was his career until ill health compelled him to retire from business in 1923. He had always been interested in anthropology and prehistory. Early retirement with sufficient means to travel gave him the opportunity to develop these interests.

For a time Noone settled in France, and through helping in the excavations of palaeolithic sites in the Dordogne in the pre-war decade he became closely associated with Denis Peyrony, Gabriel Leyssalles, Dr. Cheynier, Mr. H. H. Kidder and Professor A. S. Barnes. He published a number of papers on burins, the most important being his contribution to the *Journal of the Institute* (Vol. LXIV, 1934, pp. 81-92), 'On a Classification of Flint Burins or Gravers.' This classification, replacing that of Bourlon (1911), was based on a study of technique, that is to say on the process of production rather than on the shape of the tool. It was the outcome of the author's own experience in making such implements. Noone emphasized that the burin's essential feature was a restricted working edge made at the end (or angles) across the depth or thickness of the flake instead of along the margin as in knives and scrapers. His classification of burins into three orders, *Spalled* (A), *Scaled* (B) and *Fluted* (C), with 16 standardized types, A.I-VII, B.I-VII and C.I, II, each divisible into four, five or six varieties, is both more practical and more comprehensive than any that has been proposed before or since. In a later paper on burins in the *Proceedings of the Prehistoric Society*, Vol. XVI, 1950, pp. 186-91, Noone proposed alternative names for the three main orders or classes: *Simple* (A), *Composite* (B) and *Groover* (C). His last paper published in the same periodical (Vol. XIX, Part 1, 1953, pp. 116-120), included a classification of burin spalls, and some observations on the small *noailles*

variety of concave-scaled burin. Collections of burins classified by Noone have been deposited in the Departments of British and Mediaeval Antiquities and of Palaeontology in the British Museum.

After visiting their elder son in Malaya shortly before the Second World War, Mr. Noone and his wife, Mrs. Nora Noone, moved to Ceylon where they collaborated in an investigation of Late Stone Age sites on the surfaces of knolls near Bandarawela. The majority of the artifacts at these sites are of rock crystal, and in their description and classification of the material collected, the Noones noted the extent to which raw material determined technique. For example they observed that: 'The schistosity of certain grades of quartz caused angular and flat cleavage, which though often troublesome could be turned to account. By holding a nucleus with one of these natural cleavage planes in line with the direction of force applied by the knapping tool it would serve the purpose of lengthening the flakes, especially if the surface tension and other planes of cleavage, if any, were not antagonistic.'

A number of types and varieties of implements not recorded by previous investigators were recognized among the 2,000 artifacts collected by the Noones, who treated them as 'provisionally of one culture,' for convenience designated 'Bandarawelian,' although they were fully aware that future work on stratified sites in Ceylon might show the necessity for subdivision. The artifacts vary considerably in size, but the microlithic elements including geometrical pieces are the most distinctive part of the culture. The beautiful series of Bandarawelian microliths in 'dew-drop clear crystal' figured in the Noones' paper in the *Ceylon Journal of Science*, Vol. III, Part 1, 1940, pp. 1-24, is in the British Museum.

Noone lived in South Australia for several years during the war, and according to Mr. Norman Tindale, Curator of Anthropology in the South Australian Museum, he greatly helped the Australian archaeologists to understand the nomenclature of stone implements used in Europe. He presented to the South Australian Museum a rich collection from French sites, and worked extensively on the stone artifacts in that museum with Mr. H. M. Cooper. He published several papers in conjunction with Dr. T. D. Campbell on the stone implements of the south-eastern part of South Australia, and one with Mr. Norman Tindale on the finding of a hoard of knapped flint flakes near Eucla (*Trans. Roy. Soc. S. Australia*, Vol. LXV, 1941, pp. 116-122). He was elected a Fellow of the Royal Society of South Australia, and contributed lectures at the meetings of the Anthropological Society of South Australia. He assisted Mr. Frederick D. McCarthy in the preparation of the work entitled *The Stone Implements of Australia*, a Memoir of the Australian Museum (1946). He was the first to recognize examples of the micro-burin



('with its characteristic stigmaté,' the positive bulb on the burin facet) in collections from South Australia, New South Wales and Java. Commenting on Noone's period in Australia, Mr. Tindale said that 'Australian students in the field of archaeology gained much from his quiet influence.'

After returning to Europe he contributed a paper to *MAN* (1949, 146) on 'Some Implements of the Australian Aborigine with European Parallels.' For reasons of health he generally found it necessary to spend the winters in the South of France, and with a revival of his interest in the French Upper Palaeolithic industries he published a number of papers on burins, and one in conjunction with Peyrony on Upper Palaeolithic implements with blunted edges, in the *Bulletin de la Société préhistorique française*, Vol. XLVI, 1949, pp. 298-301. In the post-war years he generally spent part of the summer in England and it was during this period of his life that I got to know Mr. Noone. He was a welcome visitor 'behind the scenes' at the British Museum (Natural History), to which he presented a number of specimens, and personally I owe much to the discussions which we had on the occasions of his visits to London.

Mr. Noone had two sons and two daughters. Both sons became anthropologists. The elder, Mr. H. D. ('Pat') Noone, was Field Ethnographer of the Federated Malay States Museum, and later Curator of the Museum near Taiping; he disappeared during the Japanese occupation (see the obituary notices by Sir Richard Winstedt and Mr. Miles Burkitt in *MAN*, 1949, 66, 67), a tragic loss which his father found hard to accept. The younger son, R. O. D. Noone, followed his brother's footsteps after the war, becoming Adviser for the Aborigines in the Federation of Malaya, and is now Director of the Federated Malay States Museum.

Mr. H. V. V. Noone will be deeply missed by his friends in many parts of the world, and he will be remembered not only for his anthropological insight and knowledge, but for his self-effacing modesty, quiet humour and never-failing kindness and generosity.

KENNETH OAKLEY

4 Les Eyzies in the thirties must have been one of the most stimulating places in the world for the young English-speaking student of archaeology or primitive technology who was lucky enough to meet Homer Kidder, Alfred Barnes and Herbert Noone, each with his own different type of heterodox originality and all imbued with the passionate (though in Kidder's case very gently expressed) enthusiasm of the true creative amateur, an enthusiasm which would put to shame many a puny latter-day professional. Noone was a first-class flint-knapper and a genius at teaching the craft. I remember how, one day in (I think) 1938, after my brother and I had spent a happy morning at his feet, I was sitting, with a piece of flint still in my hand, among a group of distinguished French prehistorians in front of the Hotel les Glycines, while a revered authority was upholding and demonstrating his tenaciously held view that (1) making a burin was a matter of great difficulty and skill, and (2) there was only one possible method of doing it, the singularly awkward one of resting the prepared flint on the anvil and striking its butt end with a hammerstone so as to detach a burin spall against the anvil. Noone, as I knew, was one of the few people who knew better than to accept this dogma, and I could not resist picking up a suitable pebble for a hammer and with one fortunate blow striking off a perfect burin—by the direct method. He was not himself present, but he had certainly guided my hand, and I felt more proud of being in this small way the instrument of his vindication before the orthodox than ashamed of my tactlessness in bringing that part of the conversation to so sudden a close.

It is greatly regrettable that even now—perhaps now more than ever—nearly all archaeologists and other students of material culture remain ignorant of the paramount importance of an understanding of practical technology in their studies; and that many a sumptuous volume is written to little purpose for the lack of insight such as Noone's.

WILLIAM FAGG

## CORRESPONDENCE

**The Art, Myth and Symbolism of Arnhem Land.** Cf. *MAN*, 1957, 241

5 **SIR**,—Dr. P. M. Worsley's review of Vol. I of the Records of the Arnhem Land expedition, viz. *The Art, Myth and Symbolism of Arnhem Land*, greatly surprised me, because it revealed his apparent inability to understand the purpose and subject of the book and his foolishness in dismissing the book as practically valueless because it did not measure up to the false standards set by himself as a social anthropologist.

Nor was the quality of Dr. Worsley's review enhanced by his seeming presumption 'that the whole purpose of the expedition... was quite misconceived.' The reading of the introduction should have made it quite clear to him that the purpose of this American-Australian expedition was research in the several fields of natural history, the health and nutrition of the native people and certain aspects of aboriginal life, as well as the fostering of goodwill and friendly understanding between the American and Australian scientists. The success or failure of such far-reaching objectives could scarcely be measured by the content of the first of four large volumes.

No one is more aware than I of the gaps in the information which I was able to gather, or of the urgent need to fill those gaps while the opportunity to do so still remains. But I am also aware of the great disservice to anthropology that has resulted from the formulat-

ing of theories based on incomplete or insufficient field data. And so I contented myself with publishing 'an old-fashioned collection of ethnological facts.'

It is perhaps regrettable that I did not rise to the height which Dr. Worsley apparently expected of me, that of being a socio-anthropological theorist and an art-evaluator. But I did not aspire to those heights; all that I set out to do was to collect, honestly and accurately, such data as were available on the art, myth and symbolism of the aborigines of Arnhem Land, and to record those data, free from academic bias or any attempt to make those facts fit into any preconceived theory.

London, N.W.

CHARLES P. MOUNTFORD

### Diffusion

6 **SIR**,—Mr. Adrian Digby begins the chapter which he contributes to *The Scallop* (London, Shell Transport and Trading Co., Ltd., 1957) by saying: 'Up to the time of Columbus, America was not just 3000 miles from Europe—it was another world. It has been suggested many times, I know, that the earlier civilizations there may have had some earlier contact with Europe but there is no real evidence in support of this, or to contradict the view that the Continent was virtually isolated from the Old World from the time when the first Americans crossed the Behring Straits from Asia to the discoveries of Columbus.'



There is no evidence for contact between Europe and America before Columbus, and so far as I know no diffusionist has ever suggested that there is. On the other hand there is abundant evidence of trans-Pacific contacts. The best known piece of evidence, and one which has converted at least some anti-diffusionists, is the fact that the sweet potato, a Peruvian plant, was established in Polynesia long before Columbus, but there are many more, and the long list of them recently published by Professor G. F. Carter of Johns Hopkins University is most impressive.

Anti-diffusionists have been in the habit, while averting their eyes from the evidence, of pretending to believe that all diffusionists are pan-Egyptians; Mr. Digby has started a new line in fiction by making us all followers of the fabulous Leif the Lucky.

Usk, Monmouthshire

RAGLAN

### Ashanti and Hebrew Shamanism

**7** SIR.—This short note has germinated out of Professor Dorothy Emmet's Henry Myers lecture on 'Prophets and Their Societies,' *J. Roy. Anthropol. Inst.*, Vol. LXXXV, pp. 13-24.

That Hebraisms exist in some West African religions has long been known: Egypt has been postulated as their common source. But it has not yet been recorded, so far as I know, how strikingly similar is the Ashanti to the Hebrew shamanism and how the latter is illuminated by the former.

The Ashanti shaman (*okomfo*), more often than not, starts his dedicated career on the occasion of some public religious ritual, when he is suddenly possessed by an *obosom* or *honhom* and dashes off into the bush, afterwards often reporting auditory or visual hallucinations or both. The traditional period of his sojourn in the bush—as of many other ritual periods of time in both Ashanti and Hebrew cults—is 40 days. During these he is said to be fed, if at all, by mythical beings (*mmotia*). All these features recall the beginning of the ministry of Jesus, on the ritual occasion of his own baptism by an older prophet. 'And straightway coming up out of the water he saw the heavens rent asunder and the Spirit as a dove descending on him: and a voice came out of the heavens . . . And straightway the Spirit driveth him forth into the wilderness and he was in the wilderness forty days . . . and the angels ministered unto him.'

A new *okomfo* in Ashanti is forcibly restrained, if possible, by his friends from rushing into the wilderness because there is grave danger that he will never return. Roaming in the bush that surrounds the Ashanti village in which I now write is a man who was driven there by the spirit three or four years ago. No doubt the dementing deficiency diseases pellagra and beri-beri have claimed him. He has been seen several times since—naked and with hair grown long—but he shuns human contact and runs away. This was probably the fate of the Hebrew prophet Enoch. 'And Enoch

walked with God, and he was not, for God took him'—that is, took him away to bush. Elijah appears to have had prodromal symptoms of his own final running away, which event was precipitated by a whirlwind, into the dust of which he disappeared.

It is possible to interpret the final disappearance of the prophet Jesus as in the same tradition: reviving in the rock-hewn tomb (Pilate had " marvelled if he were already dead ") Jesus spent a further 40-day period, elusively in the wilderness, but several times appeared in the early morning or late evening and took food with his friends, assuring them that he was flesh and blood and not a wraith; "when forty days were fulfilled" he gathered his disciples on a hillside seemingly as the 50 sons of the prophets had been gathered to take farewell of Elijah; in the act of blessing them he was "taken up"—probably fleetly as Elijah ran before the chariot of Ahab—up the hillside till a hilltop mist "received him out of their sight."

Mframaso, Wenchi, N.W. Ashanti

M. J. FIELD

'De Kunst van Nieuw Guinea.' Cf. MAN, 1957, 178, 247

**8** SIR.—I regret that my review of Dr. Kooijman's book should have offended him. He may perhaps think me less unfair if I correct some misapprehensions for which there are no grounds in what I wrote.

I did not impudently suggest that he is 'not interested' in certain problems: my criticism related solely to his published views. Similarly, I was not concerned with whether or not he realizes the importance of references and did not mention 'exact references' at all. I merely wished to suggest a few additions to a useful bibliography. As I am personally grateful for excellent photographs made for me at the Rijksmuseum in Leiden I am particularly concerned to point out that I did not criticize the competence of Mr. Zwanenburg. A photograph may be perfect, but its reproduction in an inexpensive book is another matter; and in the copy which I reviewed the reproductions were not of the quality that one expects from art books today.

The exclusively popular character of Dr. Kooijman's book was not stated in it, and I have to admit that I did not realize his intention. My obtuseness in this respect is the more deplorable in that Mr. Cranstone discerned that it was popular ('in no pejorative sense') without being able to read it. I was misled chiefly by a five-and-a-half-page bibliography containing 111 items in four languages, many of them published in learned periodicals (such as the *Abhandlungen u. Berichte des Kön. Zool. u. Anthr.-Ethn. Museums zu Dresden*) as far back as 1876, and some of them not to be found even in the Bodleian Library. If Mr. Cranstone computes their combined market-value I expect he will find it considerable.

RODNEY NEEDHAM

Institute of Social Anthropology, University of Oxford

## REVIEWS

### AMERICA

**Die Mayaforschung.** By Franz Termer. *Nova Acta Leopoldina*, N.F., Vol. XV, No. 105. Pp. 97-163. [1]-[71], 32 figs. on 16 plates, map. Leipzig, 1952

**9** In this excerpt from the *Nova Acta Leopoldina*, Dr. Termer gives a condensed but comprehensive survey of the whole course of Maya Research from the early nineteenth century, beginning with Alexander von Humboldt, down to 1952. Its full and accurate documentation renders it a most valuable 'companion' for Maya studies.

After a short introduction the author deals with his subject matter under the following headings: I. Geography of the Maya region; II. Language and Anthropology; III. Archaeological Research, with subsections on historical records and hieroglyphs; monuments of the lowlands before 1918, and after 1918; archaeology of the highlands; inscriptions, the Calendar and problems of its correlation; IV. Problems of the Origin and Fall of Maya Culture;

V. Ethnography; VI. Index of personal names and place names—unfortunately the page references in the Index are 2 units short of the correct figures; presumably the pagination was altered while the paper was in the press. The name 'Boddam-Ehetham' is a misprint for Boddam-Whetham.

In this monograph all substantial contributions to these different branches of Maya research are briefly reviewed in chronological order and evaluated. Few students will be likely to quarrel with the author's well-balanced judgment. It is pleasing to see the outstanding value of the work of two Englishmen, A. P. Maudslay and J. Eric Thompson, fully recognized, although the latter (who has retained his British nationality during his long residence in America) is referred to on p. 117 as an American.

The author admits that we are far from having reached a final solution of the problem of Maya origins, if indeed it will ever be solved. Even the question of priority of culture as between the Maya



and Mexico remains controversial. He points to the Chiapas highlands and the little known central region of the eastern Isthmus of Tehuantepec as the most promising fields for future research, with a view to illuminating some of these uncertainties, including the 'enigma of the Olmec' and Lothrop's so-called 'Q' complex. Dr. Termer himself favours a highland origin for the eventual 'flourish' of the Classic culture of the lowlands; for its downfall he considers that both economic and religious factors were probably largely responsible.

A table on pp. 136-139 lists 130 of the principal Maya sites, together with the names and dates of their discoverers or earliest investigations from 1550 onwards.

There seems little to criticize in this thoroughly scholarly and useful piece of work; but a few comments on minor points suggest themselves. The older name of Menché for the site now generally known as Yaxchilan (a name invented by T. Maler) deserves at least a cross-reference, if only because it was used (following Rockstroh) by A. P. Maudslay in his *Biologia-Americana* and survives to this day in the British Museum! The plaster casts made from glue moulds by the School of American Archaeology at Quirigua are described (p. 127) as 'greatly superior' to those previously made by Maudslay and his expert assistants with moulding paper and plaster. One wonders whether this is not an overstatement. Certainly those of Maudslay's casts which are exhibited in museums in England are of high quality, and sufficiently faithful to the originals to be used independently of them for a study of the hieroglyphic inscriptions. On p. 132, footnote 89 refers to only two of the five reports by T. A. Joyce and others on the British Museum expeditions to British Honduras, published in the *J. Roy. Anthropol. Inst.*, 1926-1931.

Waldeck's efforts to produce drawings of the inscriptions at Palenque are very properly appreciated (p. 111), but no mention is made of their very considerable inaccuracies, some of which had surprising consequences. His wholly apocryphal elephants' heads and trunks may have been a subconscious response to his belief that the Maya were contemporaneous with American elephants, and to his desire to supply ocular proof of this thesis. But he could hardly have foreseen that those same drawings would be used nearly a century later by Elliot Smith as conclusive evidence of Asiatic influence! It is perhaps worth mentioning that a passage in Waldeck's (unpublished) diary describes his practice of working up his drawings from rough sketches; this should be sufficient warning against placing much reliance on them. The relevant volume of the diary is preserved in the Dept. of Manuscripts of the British Museum; cf. *Brit. Mus. Quarterly*, Vol. IV (1929-30), pp. 15-17.

A new edition or a supplement to this monograph, taking account of radio-carbon datings and field research since the date of its publication would be very useful; an English translation would doubtless also be welcome in some quarters.

H. J. BRAUNHOLTZ

**Town and Country in Brazil.** By Marvin Harris. New York (Columbia U.P.) (London, O.U.P.), 1956. Pp. x, 302. Price £1 16s.

IO

There is a bigness about Brazil, which has so fascinated both adventurers and more serious writers, that the country is often presented to the outside world as nature's colossus. Yet most Brazilians, so far from being countrymen, are unashamedly city-dwellers at heart. Dr. Harris sets out to investigate this apparent paradox, and in so doing to re-examine the concepts of 'urban' as

opposed to 'rural' communities in the light of Brazilian social reality. His team of ethnologists worked for a year in the interior of the state of Bahia, studying the small town of Minas Velhas and some of its satellite villages. Minas Velhas was founded, as the name suggests, in the days of the gold-mining boom in the eastern highlands of Brazil, and soon became an important administrative centre. The boom petered out towards the end of the eighteenth century, and since then the town has been in economic and finally also in bureaucratic decline. By 1921 no less than 34 counties (*municípios*) had been created out of the territory once administered from Minas Velhas. Nowadays its 1,500 inhabitants are cut off from the economic arteries of the state, and deprived of their administrative influence over other communities, with the exception of a few small villages. In this situation Dr. Harris first describes and then analyses the continuing 'urban ethos' of the town. The description is excellent. A great deal of ethnographical detail is presented in such a way as to give a vivid impression of small town life in the Bahian interior. The analysis, however, poses a problem. Many of the so-called 'urban' characteristics of Minas Velhas are also to be found in the villages. In both types of community there is a high degree of individualism and comparatively little co-operative activity; in neither are kin groups of great significance; in neither do religious values carry more weight than secular ones; nor are the villages significantly more remote from the big cities than is the town. In only one respect can the villages be contrasted with Minas Velhas according to the criteria commonly used to distinguish 'folk' from 'civilized' communities—they are homogeneous communities, whose inhabitants all farm for a living; they lack the economic specialization and the mutually antagonistic classes of the town. Dr. Harris's analysis shows the 'folk' culture concept to be unhelpful in this context because its criteria establish only a difference in degree between the communities which he is comparing, whereas his field experience has taught him that there is, in fact, a difference in kind. The intense urbanity of Minas Velhas may be reflected in its social structure but can only, in the author's view, be understood in terms of the attitudes of its inhabitants. The sum total of these attitudes, representing the people's high valuation of the urban traits in their culture, and their desire to perpetuate them, is what he calls the 'urban ethos,' and is the principal object of his investigation. He shows how it is implicit in the pattern of Iberian-American colonial settlement, of which Minas Velhas is a typical example. It is a community superimposed on the surrounding countryside, and having from the very beginning its most intimate links with the wider economy of the cities and with the bureaucracy of an empire. Though it was not deliberately founded as an administrative centre, like so many Latin American towns, its administrative functions helped to maintain it and its urban character long after it had lost its original *raison d'être*.

Dr. Harris' preoccupation with the mentality rather than with the structure of Brazilian urbanism is a new departure. Its theoretical fruitfulness has yet to be demonstrated. However, by casting fresh light on the rural-urban dichotomy in Brazil, it makes a stimulating contribution to the understanding of Brazilian society. Moreover, it is obviously the author's hope that his study will provoke a re-examination of the urban complex in other parts of South America. It is unfortunate that in a book which is otherwise so well produced the many Portuguese words should have been unaccented, leaving them somewhat naked and unpronounceable.

D. MAYBURY-LEWIS

## ASIA

**Tribal Myths of Orissa.** By Verrier Elwin. O.U.P., 1954. Pp. lv, 700. Price £1 15s.

II

The State of Orissa (India) is yet fully to be explored ethnologically. It is gratifying to note that Dr. Elwin, a prolific writer on Indian tribes, recorded the tribal myths which are rapidly disappearing along with the changes brought by the impact of contact with the non-tribal social groups. But for his assiduous scholarship much of the presented material would have been lost in the oblivion of tribal forgetfulness. The author has scientifically presented a huge collection of folklore orally

handed down among the Bhatras, Binjhawars, Bondos, Didays, Gadabas, Gonds, Murias, Jhorias, Pengus, Juangs, Kamars, Konds, Koyas, Parengas and Saoras of Orissa. The author's wide knowledge of the tribal ways of living and thinking and his study of the Hindu mythology have enabled him to make a comparative study of the two converging cultures, mutual influences of which are obvious. I feel, however, that the author has been unduly obsessed by the fear of infiltration of Hindu mythology, which slowly but naturally has pervaded the tribal societies in all parts of India. Sanskrit civilization and tribal cultures of India are



inseparable units which together represent modern Indian culture and civilization. In my opinion publication of the original stories in tribal dialects along with English versions would have been more useful to philologists and folklorists alike.

Its author has a knack for becoming very friendly with the pre-literate people whose oral literature he has faithfully recorded both for the general reader and the tribal posterity, who will gratefully remember the services rendered by the distinguished scholar. This valuable piece of work will prove to be, it is believed, a handy treatise for folklorists, ethnologists, Indologists, sociologists and psychologists alike. The newly started Tribal Culture Research Institutes of many Indian States are sure to be immensely benefited by Elwin's work as well as by his *Myths of Middle India*. Students of folklore in particular and Indologists in general will feel indebted to its author who has not only systematically treated the material at his disposal but also has added a carefully prepared glossary of names of mythical heroes and legendary figures of the all-pervading Sanskrit civilization of India. This will greatly help such readers as have no first-hand knowledge of Indian culture and civilization. The final chapter, 'Notes on Motif Index,' is an additional attraction of the book.

M. C. GOSWAMI

**The Tibetan Book of the Dead or The After-Death Experience on the 'Bardo' Plane, according to Lama Kazi Dawa-Samdup's English Rendering.** Third

12

Ed. by W. Y. Evans-Wentz. London (O.U.P.), New York, Toronto, 1957. Pp. lxxxiv, 249, 6 plates, 5 illus. Price (in U.K. only) £1 5s.

This well-known book, now in its third edition, is again presented to the public after having first been published in 1927 (second impression 1936) and re-edited in 1949 (second impression 1951). Nothing can be more indicative of its popularity, if such factual evidence were at all needed. Actually, the Tibetan Book of the Dead, ever since it first became accessible to the Western world in its English language rendering, has made so remarkable an impression on both scholars and lay readers that it can safely be said that today it has achieved the status of a classical work.

The Table of Contents on p. xxiii (while omitting itself but mentioning the illustrations on p. xxvii) shows that this edition includes the editor's Prefaces to the Second and First Editions, the learned Foreword, written in 1925, by Sir John Woodroffe, Dr. Evans-Wentz's Introduction in 15 paragraphs, and the familiar disposition of the Books of the Bardo, namely, Book I, The *Chikhai Bardo* and the *Chönyid Bardo* (Part I, the Bardo of the Moments of Death, Part II, the Bardo of the Experience of Reality), Book II, The *Sidpa Bardo* (Part I, the After-Death World, Part II, The Process of Rebirth), followed by the Appendix and the instructive Addenda of the editor, and the Index.

Innovations are Dr. Evans-Wentz's Preface to the third edition, Psychological Commentary, by Dr. C. G. Jung, translated by R. F. C. Hull from *Das Tibetische Totenbuch*, Swiss Edition, Zürich (Rascher), 1938, and Introductory Foreword by Lama Anagarika Govinda, the well-known German Buddhist.

The latter hails Lama Kazi Dawa-Samdup and Dr. Evans-Wentz as true *lotsavas* (translators of sacred texts) and attributes the outstanding success with which their work has met, to their convincing sincerity and seriousness of purpose. Although the *Bardo Thödol* is at present widely used in Tibet as a breviary, read and recited on the occasion of death, Lama Govinda reminds us that it was originally conceived to serve as a guide for the living as well. Herein lies the justification, in his opinion, for having made it accessible to a wider public. He deprecates the idea that the ancient, pre-Buddhist Tibetan religion *Bön* has inspired much of it, for, as he writes, 'Despite the popular usages to which the *Bardo Thödol* has been put in connexion with the death rituals—and herein, probably, is discernible the only trace of *Bön* influence worth considering—the central idea and the profound symbolism of the *Bardo Thödol* are genuinely Buddhist.'

The essence of Dr. C. G. Jung's Psychological Commentary is that, if the rationalizing mind of the West has succeeded in pushing forward, with the aid of psycho-analysis, into what might be

called the 'neuroticism' of the *Sidpa* state (the animal sphere of instinct), then 'a very justifiable fear of metaphysics prevented Freud from penetrating into the sphere of the occult,' that is, into the preceding, deeper-lying layer of the unconscious, corresponding to the *Chönyid Bardo*. Some psycho-analysts do, it is true, claim to have probed back to memories of intra-uterine origin, but had the journey back been consistently pursued, despite the poorness of our existing biological ideas, it would undoubtedly have led to the postulate of a pre-uterine existence, such as is to be found in the *Chönyid Bardo*. The latter, in Dr. Jung's opinion, contains categories of the imagination, archetypes which he calls dominants of the unconscious and which constitute a layer termed the *collective unconscious*. The archetypes are, so to speak, organs of the pre-rational psyche; they are eternally inherited and their specific content only appears in the course of personal experience. The *Bardo Thödol*, Dr. Jung believes, is made up of such archetypes which are reproduced spontaneously any time and anywhere without there being any conceivable trace of direct transmission, because of the remarkable similarity of the human psyche at all times and in all places.

It is interesting to compare Dr. Evans-Wentz's three prefaces, from the point of view of obtaining an insight into the evolution of his thoughts on the editing of the Tibetan Book of the Dead. Thirty years ago, in the Preface to the first edition, he is completely self-effacing ('I have been little more than a compiler and editor,' p. xx) and merely limits himself to thanking those to whom he is indebted. The Preface of the second edition (1948) is aptly entitled 'The Message of the Book.' This, we are told, is that the Art of Dying is quite as important as the Art of Living. The malpractices of a materialistically inclined medical science unfortunately interfere in the Occident with the death-process so that it is nearly always 'aborted.' For the people of the Orient, however, the transition from the human plane of consciousness, in the process called death, can be and should be accompanied by solemn joyousness. 'Eventually,' we are assured, 'as the master *yogins* declare, when humanity shall have grown spiritually strong, death will be experienced ecstatically' (p. xvii). Finally, writing in 1955, in the Preface of the third edition, the editor feels greatly encouraged by Dr. Jung's Psychological Commentary in the belief that, with the investigation of the doctrine of pre-existence and rebirth by scientists of the West, these seem to be approaching that place, where, as with respect also to other findings by the Sages of Asia long before the rise of Western Science, 'East and West appear destined to meet in mutual understanding' (p. ix).

It is unfortunate that in the Frontispiece, Folios 35A and 67A of the *Bardo Thödol* MS. are not shown in the same way, the lower one being presented the wrong way up. Plate III, p. 21, Effigy of the Dead Person, has a Tibetan text without stops (*ts'hag-t'hung*), which makes one wonder how it could have been reproduced thus. The Pān-chen Lama is in many places (pp. 26, 113 note 3, 232) referred to as the Tashi Lama, whereas only itinerant, begging monks, or 'good luck lamas' are given that name in Tibet. The semi-reformed sect of lamaism founded by Marpa Lotsava and Milarepa is repeatedly mentioned (pp. 6 note 1, 68, 72, 79, 135 note 2, 233 note 2) in the text as *Kargyüpa*. This is a popular corruption (spelt *dKar-brGyut-pa*, meaning 'the Line of White Tradition') of the more correct *Ka-gyut-pa* (spelt *bKa-brGyut-pa*, meaning 'the Line of Secret Instruction'), and it is wrong to say, as it stands on p. 68, that it is part 'of the Red Hat School'; this name is usually reserved for the older, unreformed order of the *Nyingma-pa*. On p. 188 note 1 *wang* (power) is spelt *dvang*, when it should correctly be *dpang*, there being no letter *v* in the Tibetan alphabet.

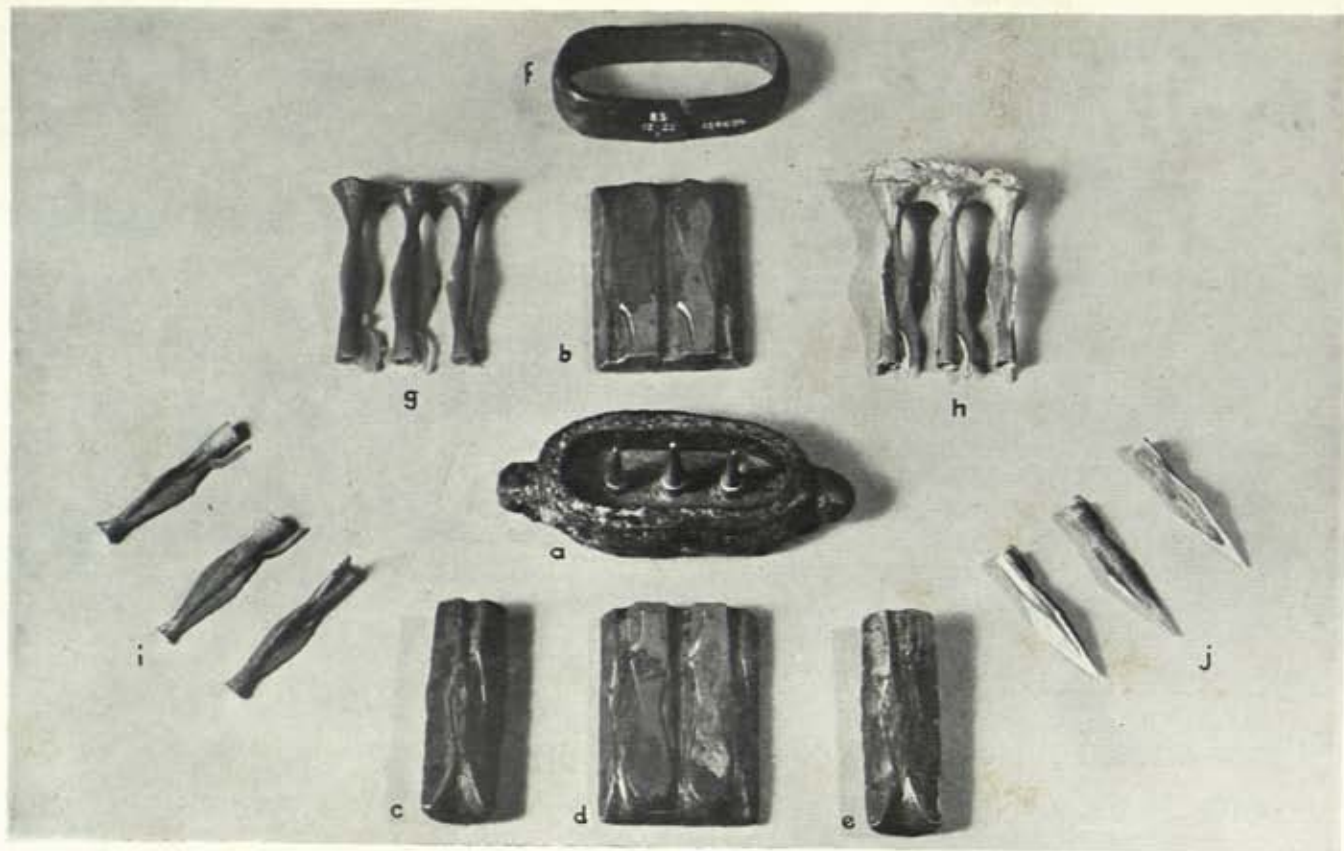
An excess of footnotes, admittedly admirably supplementing the text, does at times seem to distract, somewhat, the reader's attention from the main flow of the narrative. But this, and those other imperfections mentioned above, are only small ones which in no way compromise the very high quality of the work as a whole. The edition, like previous ones, is elegantly and attractively decorated with quotations, stanzas and citations from Oriental and Mediaeval books, and even the index at the end is terminated with an appropriate verse from Sir Edwin Arnold's translation of *The Book of Good Counsels* from the *Hitopadesha*.

PETER, Prince of Greece and Denmark









#### PART I: THE WESTERN ASIATIC SIX-PIECE ARROWHEAD MOULD

(a-f) The parts of the arrowhead mould (shown assembled in fig. 1); (g) a wax model cast in the mould; (h) the same translated into bronze by the *cire perdue* method (cf. fig. 2); (i) the same after removal of the sprues; (j) the completed arrowheads after whetting. Width of (a):  $3\frac{3}{4}$  inches



#### TO ILLUSTRATE PART III: THE CELTIC TWO-PIECE AXEHEAD MOULD

(k, l) A typical bronze mould from England in the British Museum; (m) a translation into bronze (with pouring cup and jets as yet unremoved) of a wax model taken from this mould (k, l); (n) a core mould made from a cast taken from the interior of the socket of an axehead from the Quantocks (the exact form of the mould is conjectural); (o) a plaster cast of (l) with its reconstructed core located in it; (p) a plaster cast of (k) containing one of the wax models cast in it; inside this model, correctly located, is the core form as seen in (o). Length of (k, l):  $5\frac{1}{2}$  inches

### BRONZE AGE TECHNOLOGY IN WESTERN ASIA AND NORTHERN EUROPE

All photographs by John Underwood, 1957



# BRONZE AGE TECHNOLOGY IN WESTERN ASIA AND NORTHERN EUROPE: PART I\*

by

LEON UNDERWOOD

London

**I***B The Old Fallacy.* The discovery of bronze moulds into which a bronze celt has, occasionally, been found to fit has led the archaeologist to assume that this all-purpose tool-and-weapon came to be cast in bronze moulds. The corollary of this assumption is that all such moulds are therefore older than the celts which may fit them. Even the patient and thorough student Sir John Evans, in his exhaustive study *Ancient Bronze Implements of Great Britain* (1881), believed that the evolution of technology in the bronze age depended eventually upon the usage of direct casting in bronze moulds.

With a naïve anecdote, Edward Clodd popularizes this unfounded assumption in telling *The Story of Primitive Man*, which he wrote in 1895 and based largely upon the work of Sir John Evans (to whom he dedicates the book): 'That the new is the offspring of the old is well explained in the story of the archaeologist who picked up a fine bronze celt and carried it off in high glee to a brother antiquary. The friend looked at it, and said, "Yes, it is a good specimen, and old too; but I've something older; the mould in which it was cast", and bringing out his find, fitted the one into the other.'

In another passage, Edward Clodd shows himself to be well aware of the superiority of palaeolithic man's empirical knowledge—deriving from his sense perceptions and refining his sense judgments. He speaks of palaeolithic man's struggle involving 'the constant exercise of the senses; hence the sharpening of sight and hearing, so that he could see and hear things to which civilized man, dulled by artificial aid and by less need for alertness, is both deaf and blind.'

As these palaeolithic men's tool forms and animal drawings evolved concurrently, it must follow that the sense judgment directing modification of tool forms to meet the ever changing challenge of life is in some way connected with the modifications (styles) in hunting man's animal drawings. And, granting this, it will be understood how the great extent to which our knowledge today is being transferred from reliance upon sense judgment to reliance upon intellectual judgment has obscured for us the full significance of palaeolithic man's animal drawing as also the logic of much of his tool technique.

Mere intellectual awareness of such limitations is insufficient measure for overcoming them. There is perhaps no better illustration for our lost sense judgment of proportion than in palaeolithic man's animal drawings. The truth of form in these drawings, depending entirely upon a sense judgment of proportion, may never again be equalled by the quasi-intellectual judgments of later animal draughtsmen. Does it not portend an equally acute sense knowledge

of the nature of the materials out of which the hunter formed and fashioned his stone tools and weapons?

Evans found that celts did not fit the mould at all convincingly, in the few cases where a hoard contained several celts apparently from a mould included in it. To explain this, he assumes that the mould was coated with clay on the inside to prevent fusion with the molten bronze. Of course a clay coat would serve this purpose, as he no doubt learnt from several foundrymen. But it would leave all finishing to be done on bronze rather than on wax. Without having come across a hoard of this kind, I am aware of the far greater advantage in finishing a bronze cast from a wax model which has already been trimmed of its seam flashings. Had Evans experimented in this (as he did with the manner of casting coins in the tin alloy speculum by ancient Britons) he too might have found that there was no real advantage in casting celts in bronze moulds.

The truth—as I see it, and hope to make it clear—is that many of these bronze celts may be considered older than the moulds which they are found to fit into. Put simply, the development of the bronze celt did not have to wait for the bronze mould, and therefore the bronze mould appeared, when it did, not as an improved mould for casting bronze but for another purpose. It is clearly inferior for bronze-casting. As a tool for the reproduction of the axehead form in wax it is more efficient than a pottery mould—which, no doubt, came into use as an improvement on the earliest or prototype method of forming each axehead model by hand in wax. The most likely order of increasing production efficiency is: hand modelling; pottery mould reproduction; bronze mould reproduction—of the essential wax. As bronze moulds themselves could be reproduced by the method employed for making them (and the evidence that this was done with moulds of the Celtic axeheads will be examined), clearly, when this has been often repeated, many bronze axeheads may be much older than some moulds in which they are found to fit.

Starting from Clodd's observations on our lost sense judgment, it will readily be seen how the great extent to which our knowledge has been transferred from a reliance upon sense judgment to reliance upon intellectual judgment has so cut us off from understanding primitive technology that in our reading of it men of early cultures are made to appear much more primitive than in fact they were. This translation of knowledge from terms of intuitive to intellectual judgment since the industrial revolution has interposed the barrier of specialization between the language of the scholar and that of the technician. In this light, it will be seen how the scholar's interpretation of a direct association between a celt and mould would not be acceptable to a technician in his place.

\* With Plate C and two text figures. Parts II and III will follow in the March and April issues of MAN.



This is what I believe to have happened. The archaeological student of bronze technique has for so long been unable to see how his assumption of bronze being cast in bronze moulds contradicts the view of the Bronze Age as a progressive one. To the late Professor Gordon Childe must be given the credit of first voicing a perturbation about this contradiction. In *The Bronze Age* (p. 36) he says: 'In point of fact, while some stone moulds . . . were no doubt directly employed for making the final bronze casting, the majority of them, and probably all the bronze moulds, were used not for casting proper but for forming quickly the wax model.' But this author's technical knowledge certainly could not have brought him to this conclusion, for the few technical details which he offers in support of it are without conviction of experience and sometimes could not possibly be more misleading.

The casting of my own bronzes was self-taught. As a sculptor I learned to cast without the training either of a metallurgist or of an engineer. It was the hard way—a way much closer to that in which the nature of bronze and its behaviour when used as a casting medium revealed itself to the Celtic bronze smiths who guarded it as a secret of their trade.

The sense knowledge of the nature of a material, upon which every plastic artist depends, aroused in me as a sculptor casting his own bronzes a greater interest in historical methods of bronze-casting. This first began on my visit to West Africa in 1945. While the Oni of Ife was showing me his famous collection of Ife bronze heads, I was struck by the different appearance of one—the so-called 'Olokun' head. So forcibly was I struck with its non-conformity that I wrote in my diary at the end of the day: 'Visit to the Oni . . . The Olokun head—a fake?' My reaction at this time was the more remarkable as I had not yet done any of my own bronze-casting. Any judgment I had was then based upon the elaborate amount of chasing I had done upon commercial founders' castings of my works. There is nothing like the scrutiny that a cast receives while it is being elaborately chased.

About a year later, the Ife heads came to the British Museum to be cleaned and I was able to make a more detailed examination of the Olokun head. This resulted in an article published in *MAN* (1949, 1), written in collaboration with William Fagg of the Department of Ethnography, in which were set out the several technical counts upon which the authenticity of this head was to be discredited.

As an example of tell-tale minutiae—traces left by one variation of a process posing as an earlier one—it seemed conclusive. However faint these traces may be—short of complete obliteration by erosion of time—they record some stage in the evolution of a process in use at the time and in the place in which the cast was made.

Not long afterwards, I had occasion to examine what purported to be a Græco-Roman bronze. I knew at once that it was not, because of traces left by a method of preparing the wax original which were transferred to the bronze and which could not be as early as this work's reputed origin; in this case, the flaw in the faker's knowledge of Græco-Roman technique was obvious. When the curator of the Castle Museum, Colchester, permitted me to

examine the bronze figure of Mercury from the Roman occupation which had recently been turned up by the plough, it revealed a variation of method in the handling of the original wax which corresponded closely with that of the colossal head of Hadrian from the river Thames in the British Museum. It was recognizable as a Roman technical procedure among the many variations which the *cire perdue* process underwent during its development and diffusion.

Experiences of this kind suggested to me the possibility that a comprehensible order might be adduced from the diffusion of the various methods traceable in casts made by the *cire perdue* process. Growing confidence in this idea made unacceptable the implication that a bronze socketed celt had been directly cast in the bronze mould exhibited beside it in a British Museum display case.

*The real purpose of the bronze mould.* Not being prepared to find out by making a replica and testing it to destruction, I had no idea what the life of a bronze mould would be if employed solely for the direct casting of bronze celts. What is certain however is that its life would compare very unfavourably with that of the same mould used exclusively for casting celt forms in wax.

That this was the use which they were put to I decided only after a prolonged study of many celts and celt moulds in various collections. The two-piece celt mould in bronze was a tool for the quantity production of both palstave and socketed celt, that is for the reproduction of their forms in wax—the most tedious and laborious stage of the *cire perdue* process. For my reconstruction of their use, I obtained plaster casts of two socketed celt moulds in the British Museum, from which waxes were taken and then translated into bronze in the normal way of the waste-wax process; by investing the wax totally in clay and burning out the wax to leave a one-piece or destructible mould of clay in which the bronze was cast.

Repetitions of this procedure in my studio slowly revealed the significance of every peculiar feature of these bronze moulds as well as of the interior form of their sockets. There still remained the possibility, bare though persistent, that the Celtic bronzesmiths might have evolved this bronze mould without being fully aware of its industrial capability for the quantity production of waxes. But when the Asiatic arrowhead mould was also experimented with in this same way, such a possibility was dismissed. The casting of a form in a mould of the same material raised no fences—for had it not been already demonstrated by Mr. H. H. Coghlan who had two palstaves cast from an actual ancient bronze mould (see his *Notes on the Prehistoric Metallurgy of Copper and Bronze*, Pitt Rivers Museum, Oxford, 1951, pp. 112–15)? Casting a form in a mould of its own substance was and is habitually done in plaster, gelatine and wax, when the circumstances make the use of any other mould material by the sculptor or moulder inexpedient. But no evidence can be found for such a last-resort expedient for Celtic bronzesmiths' and Asiatic arrowsmiths' use of bronze moulds for the direct casting of these tools in bronze. All observations and experiments, in reconstructing the use of these moulds, revealed that a



pretty advanced mass-production technique had been attained by both civilized and barbarian industrialists in centres as far apart as northern Europe and Mesopotamia.

The use of the arrowhead mould proved to be simpler for me to reconstruct and for this reason it is given first as simpler to describe and understand. Before doing so, however, the nature and behaviour of metal must be considered as a determinant in tool forms.

*Metal-form determinant.* Tool forms have an implacable influence on man's destiny, as Karl Marx observed in his 'technological determinism.' The unswerving purpose to improve their utility it was that has directed the succession of changes in the form of tool and weapon. This utility drive behind the varying forms of Asiatic arrowhead and Celtic axehead, behind chariots and tanks, aeroplanes and projectiles, has, with confusing contradiction, been called the 'beauty of utility'; whereas new materials and improved methods of working them have been responsible for the increasing stride and pace in the modifications of tool forms.

Laced into the destinies of nations and their cultures there is however another form which is much more free from technological determinism. The inter-relationship of the two forms is the subject of another work which cannot be even briefly outlined in this short writing, made as a study for it. The pattern made by these two forms—the practical and the spiritual—is to be seen in the perspective of history as one of a dual motif. The two forms cohere more or less together; the creative lead passing from the one to the other as the challenge of nature is met by the dual-mentality of man, and expressed by him in tool form and art form. The creative faculty of man finds expression now in tool form and now in art form.

Technology advances upon the feet of war and commerce. The practical form of weapons of war is conditioned by time, which demands the immediate-replacement of old forms by new. The problem of survival by technological improvements, which the threat of war presents, accounts for the greater urgency of practical form. The following study of bronze-age technology was made with this conviction in mind. Specialized tool forms which are characteristic of mass production appear much earlier than was generally supposed. The archaeologist's greatest contribution to a living image of early industrial history is made by him in recovering and preserving the documents. The historical image of industry constructed upon a theoretical knowledge of techniques—with its inevitable omissions and contradictions in an account of 'what' has taken place without the more penetrating enquiry of 'how'—has an unreality which divorces it from contemporary experience. Without this factor of technological 'know-how' so decisive in the present drama of our cultural survival, industry in history is remote from our experience in its legendary detachment of the past. It is for those who have experience in the use of such tool forms, in their descendant forms of today, to breathe a living significance into them.

The encouragement which a nation has always been compelled to give to its armourers of inventive genius to

keep abreast of this threat of the times was not extended to the coiners, in the promotion of commerce, until, as far as we know, the seventh century B.C.

The bronze arrowhead, even in the earliest 'unique' or 'one off' production technique—whenever that was—carried a similar threat of annihilation for established cultures to that carried by the 'one off' production of the atomic bomb. The subsequent improvement of arrowhead manufacture resulting in a mass-production technique perpetuated the threat of destruction as the stock-piling of the atomic bomb has done. A new weapon ceases to carry any decisive advantage for its users when the inventive lead in improved tools for advancing its production in greater quantities passes to another.

The evolution of practical form is determined by the tool-maker's mastery of his materials. The gradual revelation of the nature and behaviours of his materials called for improvement in his method of working them.

*The nature of tool materials.* Before examining the parts played in bronze-age technology by Asiatic arrowsmith and Celtic axesmith, it will be as well to provide the reader with a brief background sketch of the smith's increasing knowledge of his materials.

*Natural copper.* Copper began to reveal its metallic nature to the stone tool-worker who first picked up a chunk of it on the surface while searching for suitable stone. He found that it yielded to the blow on being struck, rather than chipping like stone.

*Cold working.* The stone worker also found that copper hardened and became brittle in being wrought by hammer blows. As further hammering made it split along the edges, he had to resort to his old abrasion technique for completing the form, as with stone tools. Hammer-hardened copper was no easier to abrade than the yet harder stone. He thought to remedy this by an increase of its hardness, by increasing the heat produced in the hammering. But when he took it from the cooking-pot fire he found it soft again.

*Annealing.* Our stone-tool worker had become a coppersmith when he realized his skill and re-softened the copper at will by annealing. True, the number of annealings needed to complete a copper form by hammering was prodigious. He nevertheless found the efficiency of a tool so produced such that he continued. When our coppersmith now attempted to make copper softer still by more heat than required to anneal it, the copper melted.

*Puddle casting.* On recovering the melted copper—failures in several melted tools—from the cinders on the hearth, he found the copper of all in one piece. The coppersmith fashioning his tools by annealing and hammering now became a founder, able to run small and useless nuggets of copper together into suitable lumps for tool-making.

*Open-mould casting.* The inventive smith, having thus greatly expanded the natural supply of raw material available in suitable size, set himself to improve the rather formless shapes of hollows in the hearth where the molten copper gathered by cutting shapes more precisely in slabs of stone. To fill these, he had to form a crucible of clay to receive the puddle of a melt in the fire, so that it could be lifted out and poured into the open stone mould. The open-mould cast improved upon the form of the puddle-founder's ingot for the beginning of hammered tools. The open mould halved the hammer-smith's labour in annealings.

*Smelting.* The expanding supply of copper in small fragments which puddle casting made it possible to use, could only keep pace with the growing demand by the finding of ever smaller fragments to melt. As the heat needed now to melt these small fragments together ceased to be enough when they became smaller still, our founder-smith now had to find a method of increasing the heat of his coals by a forced draught to melt even the smallest fragments embedded in minerals as ore. When ores with visible metallic



grains were exhausted locally, he tried those same minerals bearing only copper salts which he recognized by their colour. His skill now as a smelter-founder-coppersmith became a greater mystery to his fellows, and he guarded its working 'know-how' from them as a craftsman's secret.

*Alloys.* The toolsmith, with more experience of ores, now sought for those which in reduction by smelting gave a copper which was both more liquid in the crucible and harder when cast. He was in fact broaching the detection of alloys in natural ores.

*Geology.* In looking for alloyed metal ores of this superior quality, he recognized them by colour and form, and was soon able to improve the softer metal yielded by one in blending it with another ore yielding harder metal in the smelting furnace.

*Metallurgy.* Now he could either blend the ores in a smelt, or smelt them separately and blend them in the crucible. He could in fact increase those small percentages of tin or silver in ores yielding natural or poor bronze and in time produce an artificial or true bronze.

*Cire perdue.* The simplest and most direct means of making a one-piece mould in loamy clay was now within his reach. True bronze was liquid enough to be poured into an enclosed cavity of much finer form and proportions than was hitherto possible for the natural or poor bronze. His much improved understanding of the nature of metal prompted him to think of this kind of wholly enclosed mould.

*The one-piece mould.* The clay which he used for puddle moulds might be wrapped round anything in one piece. But to get the pattern object out meant cutting the mould open in two or more parts, until of course he made it in wax and then ran it out through a small hole on heating the mould. The one-piece mould of the *cire perdue* process minimized post-casting operations, for all trimming off of metal seams, and so on, might now be done in wax. But it meant the tedious repetition of modelling by hand in wax each tool form to be cast!

*The two-piece mould.* The obvious thing to do was to return to the two-piece mould, but now not made of loam for casting bronze but of fired clay or pottery for casting any number of a particular form required in wax. Post-casting operations in removing seams and cleaning up the bronze could still be much more easily done on the wax cast. More important, the labour of modelling each wax had been eliminated by the use of the two-piece pottery mould for reproducing the wax form.

*Metal to wood.* A bronze tool of desired form could now be reproduced with all its finish by casting first in wax and then in bronze. But this had not solved—for the bronzesmiths of Northern Europe anyway—the problem of a more efficient mechanical union of bronze head to wooden shaft or handle. This was an old, old mechanical problem which continued to press ever on them until the end of the bronze age in Northern Europe.

Our background sketch has now approached that state of progress which will be followed in greater detail in our consideration of the bronze moulds which we possess for the Asiatic arrowhead and the Northern European axehead; for by this time, when the smith had acquired so much understanding of the nature and behaviour of metal, from copper unalloyed to copper alloyed as bronze, his further progress in tool-making depended less upon metallurgy and more upon improvement in mould design. This involved a closer understanding of mould-making materials.

Before closing this writing for the press, however, I was much gratified to read Professor F. C. Thompson's article in the January issue on 'The Early Metallurgy of Copper and Bronze' (MAN, 1958, 1). The author determines upon a definite sequence in the ancient smith's revelation of the nature and behaviour of copper, its alloys and their ores. This he adduces from a tabulated series of analyses of copper

and 'bronze' objects of various dates. His writing informs me of *what* the early smiths and their co-workers did to obtain raw material—in a field in which I have no practical experience or 'know-how.' *What* the smiths did with the raw materials so reduced from ores is pretty well represented by surviving works, but as to *how* it was done, I believe this to be more readily recoverable in the practical experience of the sculptor-founder than in that of the industrial founder, who has so vastly transformed original techniques by his commercial 'progress.'

*The bronze arrowhead mould.* Arrowheads and rifle bullets are expendable missiles. The wings have been removed from the latter and the gun barrel rifled to keep it point foremost. The intercontinental rocket returns to the winged form of the bronze arrowhead, but unlike it, it carries its own propellant. War has ever made the most insistent demand for invention on armourers, demanding from them ever greater range combined with lethal power and increased quantity production.

Three-winged arrowheads cast in bronze have turned up in Sumerian excavations. But these tell us little about their manufacture. The possession of a six-piece bronze mould, in the Western Asiatic Antiquities Department of the British Museum, for the production of three conjoined arrowheads, tells of the efficiency of arrowhead-production. This mould (see Plate Ca-f and fig. 1; also described by Mr. H. H. Coghlan in MAN, 1952, 245) is ascribed to the Scythians of the sixth to fifth centuries B.C. It is a remarkable hand-made precision tool. Each piece is designed for studied economy in handling as the operator assembles and dismantles the pieces in quantity production.



FIG. 1. THE SCYTHIAN ARROWHEAD MOULD ASSEMBLED

But was it intended for casting directly in bronze? Such a supposition, I felt, contradicted the pointed streamlining of its parts for speedy operation. The rate of production by it if used directly for casting bronze arrowheads, would be hardly more than four casts per hour, or 12 arrowheads. On the other hand, using this actual mould for casting arrowheads in wax, I was, without previous rehearsal, able to turn out casts in wax at the rate of 60 per hour, or 180 arrowheads. I proceeded further: taking one wax or three arrowheads (Plate Cg), I invested it in clay (grog), burnt out the wax, and poured in the bronze (Plate Ch).



For this purpose, the smallest feasible quantity of bronze (one and a quarter pounds) was melted in a jeweller's crucible (to be illustrated in Part III). The rapidity with which so small a melt cools on being lifted out of the furnace meant pouring it with the least possible delay. Yet the three bronze arrowheads (Plate Ch) took only about one-sixtieth of this small melt. I then took a descending stem or sprue of wax and arranged nine triples or 27 arrowheads in wax around it as in a bunch of grapes and poured this group (fig. 2). No care was taken to skim off dross from the crucible surface; the rapid cooling did not allow time for this precaution. All but one of the 27 on the bunch were successfully cast; dross had entered the neck of this one and prevented it from filling. The allowance of wax runners and jets had been liberally heavy. Had I made these with the maximum economy of wax my one pound and a quarter of bronze might well have cast 90 arrowheads, and, with the runners from them returned to the crucible, another 60; i.e. 150 arrowheads in a pound and a quarter.



FIG. 2. TWENTY-SEVEN ARROWHEADS CAST EXPERIMENTALLY IN A SINGLE OPERATION

Finally, I adduce the trunnions on the base plate of this mould. These features (Plate Ca) are to be regarded as an added accessory for mass-production operation and they give a particularly modern touch to the *ensemble*. They speak of a long-experienced hand manipulation of this type of arrowhead mould—of time lost in dismantling and re-assembling the pieces entirely by hand—for they are an addition making these operations semi-automatic. The trunnions were engaged with fixed sockets so that the mould

could be turned upside down by a flick of the operator's finger. With it thus suspended, he might take off the keeping (Plate Cf) with one hand and catch the falling four principal pieces (Plate Cb-e) in the other hand, set aside the wax released by them, and set the base plate upright again on its trunnions, ready to receive the reassembled parts and a refilling of wax.

*Specialized tool form.* A tool form specialized for mass production was suggested by the ready way in which this mould operated. At first, it posed the question why the centre arrowhead had only two wings while those on each side had three. When it came to sharpening the blades of the wings this question was given more point; for the three-winged arrowhead was sharpened by three whettings whereas the two-winged one needed four whettings. The sacrifice of efficiency in one additional whetting for the two-winged arrowhead discloses how well considered the design had been. The addition of a third wing to the centre arrowhead would involve five additional pieces in the mould, or by a different arrangement four pieces. Time saved in assembling six pieces instead of 11 (or ten) was the answer to this question.

The progression of quantity production, restated for clarity, finds a half-cast half-wrought arrowhead preceding the wholly cast progression, which begins, in Stage One, with unique or hand-modelled waxes. Stage Two succeeds this with reproduced waxes from (probably all) pottery moulds. The pieces from Ur (at least 2,500 B.C.) are in this stage. Then comes the ultimate streamlining of reproduction technique in the mass-production technique of this Scythian mould—which may well be far from the earliest representative of its specialized type. The following experience gives me confidence in this: I cut a replica mould of the centre (or two-winged) arrowhead in Plate C. As I cut deeper and wider on the mould I proofed the form for continuous wax-production, and found that imperfect wax forms only were obtainable up to the very last when in shape and depth my replica was given exactly the internal proportions of the Scythian original. (This replica will be used for observing the effect of repeatedly pouring bronze into it.) The section of the Ur arrowheads shows the unawareness of form-and-flow and heat-transfer economy and they were probably cast with *natural* cores (to be explained in Part III of this paper).

*Heat exchange.* The greatest possible production implies a knowledge of heat exchange in the designer's experience. I worked with a microcrystalline wax of about the melting point of beeswax—140° Fahrenheit. The rapid loss of heat in the wax to the metal parts of the mould prevented perfect casts from being taken until the metal parts had warmed up and allowed the wax to flow in the mould. When the mould temperature had reached an optimum, the transfer of heat from wax to mould to air was so balanced that continuous operation was easily maintained. The thickness and balanced proportion of the pieces of the mould had been calculated by experience of this heat cycle under a continuous production of waxes.

*Wetting agents.* The advantage of this highly specialized mould form for mass production would have been of no



avail without other refinements in the production being provided for in after-treatment of the waxes. Each arrowhead has a delicately tapered bronze peg on the base plate of the mould. These pegs form socket cavities in the wax casts, the centre two-winged cavity being the deepest. While such cavities might be filled with clay in a slip condition, much care would be needed to exclude the air trapped inside them. It cannot therefore be assumed that the time needed to do this in the case of jewellery could be afforded in arrowhead-production. In order to eliminate this tedious delay a wetting agent would be used to reduce the surface tension of the water in the clay slip and to wet the surface of the wax. The production efficiency of the mould would be pointless unless all post-wax-casting operations were equally efficient.

*Functional design.* The core of clay filling the socket cavity of the Asiatic arrowhead stood vertically upon its base inside the mould, its point vertically over the base so that the molten metal could flood down from the sprue above the point, delivering equal pressure all round it, and so avoid breaking it in the undirected rush of metal. The air found its way out through the pores of the clay mould. The sprue was tapered above the point of intersection of the wings to a narrow which facilitated the removal of each arrowhead from the rest by cutting with a chisel. This arrangement ensured that the delicate barb would also fill, and that the whole would solidify on cooling from the bottom upward. The thicker sections, cooling last, would supply liquid metal to replace loss due to shrinkage and prevent cracking or fault of spongy metal. (In the socketed celt, to be described in Part III, this order was reversed. At the top was the socket with its much stouter section and the metal flowed down one side of the core suspended within the mould.)

*Form and flow.* The flow of wax in the bronze mould is slowed down as it loses heat to the walls of the mould. The form of its interior will produce a similar effect upon the flow of molten bronze in the clay mould replica of its form.

It is not at all likely that this time-saving tool for the reproduction of waxes was designed by craftsmen ignorant of the close correspondence between the effect of the form in bronze upon the flow of wax and that of the same form in clay upon the flow of bronze. Despite the great difference of comparative temperature between wax and bronze—something like 1050° C.—the flow of bronze could be regulated to that of the wax by raising the temperature of the clay mould when burning out the wax. The passage of the fluid wax/bronze might be varied in the mould form, so long as the gates or narrowings were short enough not to reduce the fluidity of wax/bronze and prevent it from reaching the bottom as it flowed down the sides of the core to make a liquid reunion there.

*Division of labour.* My brief experimental work had informed me that, were I an experienced Asiatic arrowsmith with a bronze mould like this one—and a few skilled assistants—I might confidently undertake to deliver 10,000 arrowheads in a week. The 5,000 archers of an Asiatic army might well be provided with a hundred arrowheads each, or half a million in all, in a month by half-a-dozen smiths' shops with similar moulds (524,160 might be cast from two tons of bronze).

At a guess, the craftsmen engaged in each separate operation under the master arrowsmiths may well have been: 1, a wax-moulder; 2, a wax-finisher with a hot tool joining the waxes up for group casting; 3, a mould-maker, dipping the wax groups into a clay slurry and thickening them up as they dried out; 4, a foundryman putting the moulds through a fire to burn out the wax; 5, a second foundryman melting the bronze and pouring the moulds; 6, a fettler, breaking open the mould when cool and cleaning off the clay; 7, a finisher, cutting off the jets and runners to be returned to the crucible and sharpening the wing blades by whetting them on a sandstone. The finished arrowhead (Plate Cj) would now be ready to be fitted with flighted shafts produced by another specialized industry.

## SCIMITARS, SABRES AND FALCHIONS\*

by

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**I**4 This is an attempt to clarify thought and expression with regard to a certain particular weapon type, by defining the purpose which governed its basic form, even though this might from time to time assume a different appearance. The whole range of this weapon type can be covered by three names, each contemporary with progressive alterations in shape, which are *kh.p.sh* (vocalized *khopesh* or *khepesh*), *machaira* and *yataghan*. The first two are as a rule translated as scimitar and sabre respectively, and a *yataghan* being of more or

\* With three text figures

less contemporary use is a Turkish weapon well known by that name.

Sir Flinders Petrie in his book *Tools and Weapons* puts the matter quite clearly. Of the *kh.p.sh* he says: 'the peculiarity of the type is the deep hollowing of the back, and the projecting of the edge far in advance of the handle. By its great curvature it was intended for a wiping cut.' Of what he terms the 'Recurved Knife' Petrie says: 'The motive of it is to combine two forms, a convex edge for a wiping cut and a hollow edge for a heavy cut.' Mrs. Maxwell-Hyslop, dealing with the *kh.p.sh*, states: 'In its most



primitive form it must have attempted to combine the advantages of the battle-axe and the cutting sword,' and 'to protect the hand the curved edge of the blade was projected far in advance of the handle.'<sup>2</sup> Now these are the points that characterize the style of weapon with which we are dealing: the combination of convex and concave in the blade, with the leading, convex, portion projecting ahead of the user's hand when gripping the hilt. So it will be as well to state at once quite categorically that this wholly precludes the use of such terms as *Sichelschwert* or Sicklesword when speaking of this class of weapon.

*Sichelschwert* and scimitar are not synonymous. The *kh.p.sh* type of sword is seldom sharpened on the inner or concave portion, in fact only one such actual instance is known. But one must not think in terms of superficial appearances; a sickle shape does not make a sickle-sword, which presumably indicates a sword the concave margin of which, like that of a sickle, was sharp and used as the effective cutting edge.<sup>3</sup> To follow the development of this type, designed primarily as a cutting weapon, let us use the known contemporary terms of *khopesh* and *machaira*, a justification for vocalizing the former in this way being proposed below.

There are indications from monuments in Babylonia that the earliest form of the *khopesh* was a curved wooden blade armed with sharp flints, as a crook-headed weapon or sceptre held by Ashur-nasir-pal has flanges indicating this ancient use of flints. Curved objects in the hands of leaders or deities on the earliest sculptures, such as that held by Eannatum in his chariot on the Stele of the Vultures, are clubs, batons (insignia) or even whips but not a form of *khopesh*. To such extent as is possible, account should be taken only of actual weapons surviving and not of artistic representations, which suffer from a number of handicaps, inexact copying and the limitations imposed by the object decorated being the most common.

The earliest actual *khopesh* yet found are those unearthed by Léon Heuzey at Tello in Babylonia. These came from 'un tombeau de terre cuite en forme de tonneau [qui] mettait de nouveau en présence les deux modes de sépulture qui paraissent avoir été usités concurremment à la même époque.' Of the two, one was sharp on both sides (as remarked by Mrs. Maxwell-Hyslop, this is exceptional), the other was sharpened only on what Heuzey calls 'sa courbe extérieur,' i.e. its convex margin.<sup>4</sup> It will be seen that all other known *khopesh*, whether actual or representational, have the sharp edge on the convex side. Though sickle-shaped these were not sickle-swords.

What do the surviving examples tell us that might give some clue to the dating and distribution of this rather peculiar weapon? The earliest known specimens, just mentioned, are from Tello, where similar weapons are also depicted on monuments. An Early Dynastic period date (earlier than 2340 B.C.) has been suggested for them, but though this may prove correct, the one of the more normal type with a single edge is as developed as, or more so than, ones found at Byblos and Sichern (fig. 1a). These latter are similar *khopesh* of rather primitive form, both having inlaid decoration, the one from Byblos an uræus and that from

Sichern a lotus flower: both are dated to the period of Amenemhat III (c. 1820-1776 B.C.).<sup>5</sup> It is difficult to believe that the examples from Tello can be 500 to 600 years older than this, though it is astonishing how many weapon types seem to have evolved in Babylonia during the period Early Dynastic III which are not traceable elsewhere for about 300 or more years.

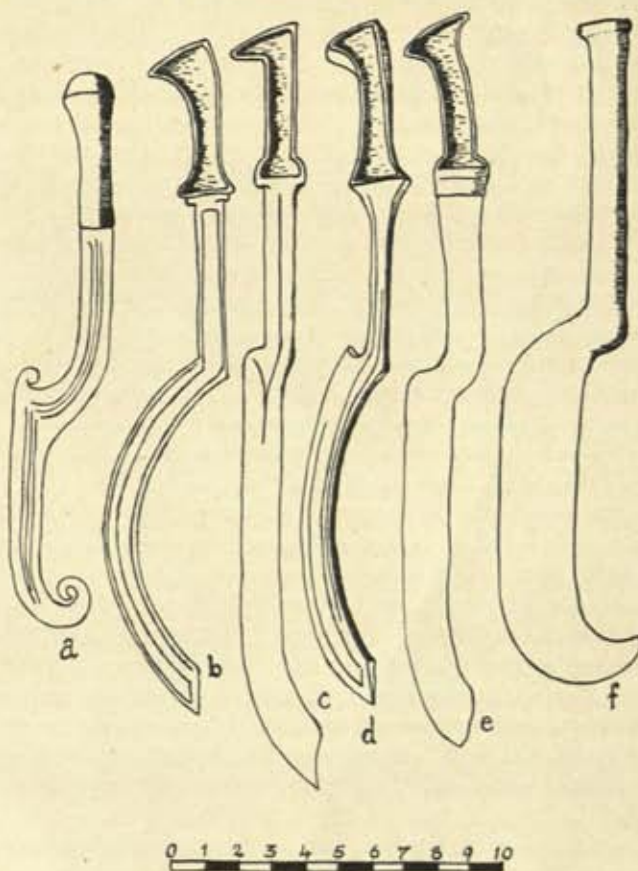


FIG. 1. EXAMPLES OF THE 'KHOPESH'

(a) Tello (after Heuzey); (b) Gezer (after Macalister); (c) Phœnicia, possibly Byblos (after Dussaud); (d) Ras Shamra (after Schaeffer); (e) Tell Retabeh (after Petrie); (f) Hittite, Yazilikaya (as indicated by sculptures). Drawings by D. H. Gordon. Scale in inches

Then there follow a number of *khopesh* all of which can be linked as being roughly contemporary within a period of 100 years by the fact that they have an identical style of flanged hilt, with a projection towards the sharp edge of the blade, the whole cast in one piece. The earliest, that from Tomb 30 at Gezer, is dated as earlier than 1425 by Schaeffer, but the fourteenth-century dating proposed by Mrs. Maxwell-Hyslop may be closer, as this *khopesh* was accompanied by what may be a buckle prong of the kind shown by Dörpfeld as coming from Troy VII.<sup>6</sup> Similar flange-hilted *khopesh* come from Phœnicia (Byblos?), now in the Louvre, unstratified but probably fourteenth-century, and from Ras Shamra, also fourteenth-century (fig. 1b-d).<sup>7</sup> From Diarbekir in eastern Turkey there is the inscribed scimitar of Adad-nirari I (1304-1267 B.C.), and from Tell Retabeh in Palestine, a *khopesh* of the time of



Rameses II (fig. 1e) (1292-1225 B.C.), which shows a continuance throughout the thirteenth century to link with examples of the early twelfth century, all known from depictions. One of these is on an ivory plaque from Megiddo (c. 1200 B.C.),<sup>8</sup> on which it is shown carried by a foot soldier; for the rest, there are four examples illustrated by Petrie from monuments of the XX Dynasty; whether or not any of these represent actual contemporary weapons, there is no direct evidence for the use of the *khopesh* after 1150 B.C.

The *khopesh* is called 'harpé' in French archaeological literature; Schaeffer calls the Gezer example 'une belle harpé,'<sup>9</sup> and the one from Ras Shamra is captioned as 'Harpé en bronze.' This is an application of the Greek word ἄρπη signifying a sickle, which, as these scimitars are sharpened on the convex side, constitutes one of the many misuses, as regards the weapons we are discussing, of words whose primary meaning is sickle. The word ἄρπη is associated in Greek mythology with the weapon of Perseus which was traditionally a sickle of ἄδᾶμος. The rescue of Andromeda by Perseus has since classical times been a favourite theme for artists, and this has produced a whole series of weird and wonderful *harpés*, all of which emanate from the imagination of the delineators.

A similar weapon appears in the Hittite country at much the same time. Rock sculptures of processions of warriors at the Hittite site of Yazilikaya show them as carrying weapons of the *khopesh* type, having the edge of the blade well ahead of the hand (fig. 1f).<sup>10</sup> These appear in both the main and the side galleries and so lie within the period 1275-1220 B.C. This does nothing to bring this weapon type any lower in date than those already mentioned, but it does extend its use to Anatolia, a point of some significance with regard to our next examples of weapons popularly referred to by words associated with sickles.

Some time in the first half of the sixth century B.C. a weapon was adopted by the Greeks which they called μάχαιρα or κόπρις. The *machaira* is in fact mentioned by Homer, but in circumstances indicating that it was a knife of no great size. The representations of this knife on Greek black-on-red vases generally show it in domestic use as a kitchen chopper, but its size is such that it already had all the makings of an effective weapon. Though its depiction as a weapon on black-on-red vases is very infrequent and dates not earlier than 530 B.C., the appearance of this type is increasingly common on red-on-black vases from c. 510 B.C. onwards.

The *machaira* is essentially that weapon which Petrie calls the 'recurved knife' and which combines both convex and concave edges along one margin of the blade, having the convex portion in advance of the hand when gripping the hilt. It was distributed all over the Balkan peninsula, and travelled to central Italy, south-eastern Germany and Spain. It is of the type that has usually had the name of sabre attached to it for convenience. In Spain, however, where as regards surviving examples it is particularly common, it is called the '*espada falcada*,' thus indicating that it was a sickle-sword, whereas in fact its shape cannot be said to bear the slightest resemblance to a sickle. The

earliest known depiction of the *machaira* is on the Harpy Tomb from Xanthos in Lycia (c. 540 B.C.), and an origin in Asia Minor appears likely.

There are in fact indications with differing degrees of value that this suggestion of Asia Minor as the likely place of origin may be correct. It seems that the areas in which the *khopesh* was prevalent produced two weapon types, both of which acquired sickle-shaped connotations. It is also for consideration whether the word κόπρις might not be derived from *khopesh*. Its normal derivation is from κόπτω, but it could have been popularly connected with κόπτω as cutlass is often and wrongly with cutting. Sir John Myres says of the word *sft* which the Egyptians used for a long straight sword: 'If the Greek *xiphos* [ξίφος] is also connected, *sft* may originally have been a Libyan or Sea-raider word.'<sup>11</sup> Similarly the κόπρις which embodies the same principles as the *khopesh* may also be derived from it in name.

When Herodotus in Book VII described the equipment of the various contingents of Xerxes's army, he says of the Egyptian seamen (89, ii) that they all had long *machairai*, which Rawlinson, having in mind boarding parties of jolly Egyptian Jack Tars, quite naturally translates as cutlasses, and he tells us (91, iv) that the Cilicians were armed with a sword closely resembling the *machaira* of the Egyptians. On the other hand he states that the Lycians and Carians (92, vi, 93, viii) were armed with the δρεπάνη, which Rawlinson translates as falchion, and this brings it into the same general category that we are discussing.

What have we in the way of actual surviving examples of the weapons that the Greeks called *machaira* and *drepane*? As has been mentioned, there are plenty of instances of the *machaira* being used to deal a slashing stroke depicted in vase painting, but the swords themselves are very scarce. There is an example in the National Museum at Athens from Epirus, which Pierson Dixon states to be the only one found on Greek soil; it is the stock illustration.<sup>12</sup> In the *Reallexikon der Vorgeschichte*, Vol. II, Plate XLIX, however, a sword of the Certosa period, starting c. 530 B.C., from Sanskimost on the River Save in Yugoslavia is illustrated, which is quite definitely a *machaira* 24½ inches in length, i.e. a trifle longer than the normal Iberian sabre (fig. 2a). Speaking of Iron Age finds in Illyria, Casson mentions this sword from Sanskimost as an almost typical example of a *machaira*, and observes that all the small knives found at this site are single-edged and of *machaira* type.<sup>13</sup> The blades, in his fig. 68 from Halos and Chanchitsa, which he claims as being of this type, do not resemble the *machaira* in any way.

In the collection of the Institute of Archaeology, University of London, there is a large iron knife with a broad blade, flanged along the back, a true κόπρις; which being 22½ inches long can be classed as a *machaira* sword. It was found in the high desert near Armant in Egypt and may represent that weapon used by the Egyptian seamen in Xerxes's fleet which Herodotus called *machaira* (fig. 2b).

By far the greater number of existing *machairai* are Iberian. They would appear to have been introduced some time in the fifth century B.C. by Iberian mercenaries, who



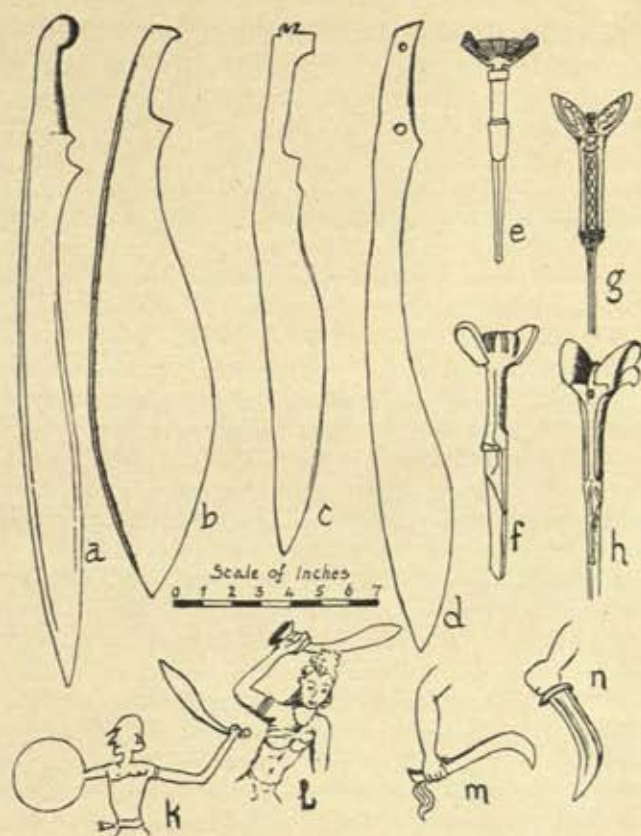


FIG. 2. EXAMPLES OF THE 'MACHAIRA,' 'DREPANE' AND OTHER SWORDS

(a) Machaira, Sanskimost (from Reallexikon der Vorgeschichte); (b) Kopis, Armant (Institute of Archaeology, London); (c) Sword, Franconia (after Petrie); (d) Machaira, Tolentino (after Petrie); (e, f) Sword hilts from Chagoula Derré, Talish and Tsalka, Caucasia (after Schaeffer); (g, h) Yataghan hilts (British Museum); (k, l) Sabres, India; (m, n) Drepane (after Texier and Hass)

had encountered them while fighting in Sicily and in Greece itself. Many such have been recovered from tombs in Spain, and they show clearly what a formidable weapon the *machaira* was (fig. 3a).<sup>14</sup> In connexion with that found at Sanskimost in Yugoslavia, Casson mentions a suggested northern origin for the *machaira*. It is true that certain iron sabres of the recurved-knife type from Traubingen in Bavaria and from Franconia are of the form which carries the convex striking edge of the blade forward of the hand, but these can be dated as Hallstatt II, Reinecke's Hallstatt D, which places them in the fifth century B.C. and it is probable that they derive from the Greek *machaira* which seems to have been spreading at that time (fig. 2c).<sup>15</sup> Examples which can be dated to this time come also from central Italy, one from Perugia in what was eastern Etruria and one from Tolentino in Picenum (fig. 2d).<sup>16</sup>

It is strange that the *machaira* did not achieve a more widespread popularity. As noted by Xenophon, it was an excellent cavalry weapon; especially at a time when mounted men were without stirrups and their horses were small. Nevertheless the fact remains that most if not all of those shown wielding the *machaira* on Greek and Iberian pottery are on foot. The Iberians used the *machaira* down

to the close of the first century B.C., and though it is probable that there was a continuity of this weapon type in the eastern Mediterranean to produce the yataghan, it cannot readily be traced.

Before turning to the question of the yataghan, we must consider the next weapon in the chronological sequence, which has been associated by etymology and by literary, and therefore popular, thinking with the sickle-sword, namely the falchion. The falchion is a literary weapon *par excellence*, keen-edged falchions abounding in historical romances. The Shorter Oxford and Murray's English Dictionaries describe it in identical terms as 'a broadsword, more or less curved, with the edge on the convex side,' which, except that it is straight, describes quite accurately one of the few existing falchions, a fourteenth-century weapon now in the Norwich Castle Museum (fig. 3b). In spite of its name, which seems to derive ultimately, through *fauillion* (known as early as 1303) and a vulgar Latin *falcion* (not known but presumed), from *falx*, a sickle, there is nothing remotely sickle-shaped about it. It is a long straight sword with its blade broadened towards the point, from which a shallow concave scoop extends back for some inches along the rearward blunted margin.

In spite of a strong resemblance to the Anglo-Saxon *scramasax*, the falchion may prove to be an oriental weapon introduced to western Europe by the Crusades. This form of blade, ending in a broadened portion with its sharp edge convex and its blunt one having a concavity backward from the point, is popular in the depiction of oriental arms and would without doubt be called a *scimitar*.<sup>17</sup> It is clear, however, that when Rawlinson translated the word *drepane* as falchion, he did not have in mind the actual historical falchion we have just been discussing, but was considering only how he could translate in weapon terms a word usually applied to sickles and pruning hooks; falchion with its *falx* derivation was the obvious choice.

What was this *drepane*? Clearly it was to Herodotus something quite different from a *machaira*, and, in spite of a general literary tradition of nonchalance in such matters, we can take it that here is the closest approach we shall get to the discredited sickle-sword. There are few representations and, to my knowledge, no actual examples of the *drepane*. If sickle-sword means one having its sharp edge on the concave of a crescentic blade curved back from its handle, then no such weapon existed in ancient times. The *drepane*, if the two sculptured examples represent it faithfully, was shaped like a pruning hook. A stele from Koniah (Iconium) in Lycaonia may well show the weapon with which the Lycians and Carians were armed. It is held by a warrior whose whole equipment in every particular is strange, and one suspects bad copying by Texier.<sup>18</sup> The other example is Etruscan; an image of a warrior in painted clay from the temple of Mercury at Civita-Castellana shows him gripping a hooked weapon of exactly the same type as that shown in the Koniah stele<sup>19</sup> (fig. 2m, n). Dated to the early fifth century, this may well be a weapon which evolved in Asia Minor during the preceding century along with the *machaira*.

The yataghan, which must have continued as a direct



derivation from the *machaira*, has a wide distribution. Primarily in recent years a Turkish weapon, it spreads from Albania to India. Though normally about 26 inches in length it may extend to 30 or slightly more, making it a cavalry sabre of great efficiency, as exemplified by the Indian example illustrated (fig. 3c).<sup>20</sup> Both the *machaira* and the yataghan are primarily designed for cutting, but in most cases they could quite well have been used with the point, and it is in this respect that they differ from the ancient *khopesh* and the modern kukri.

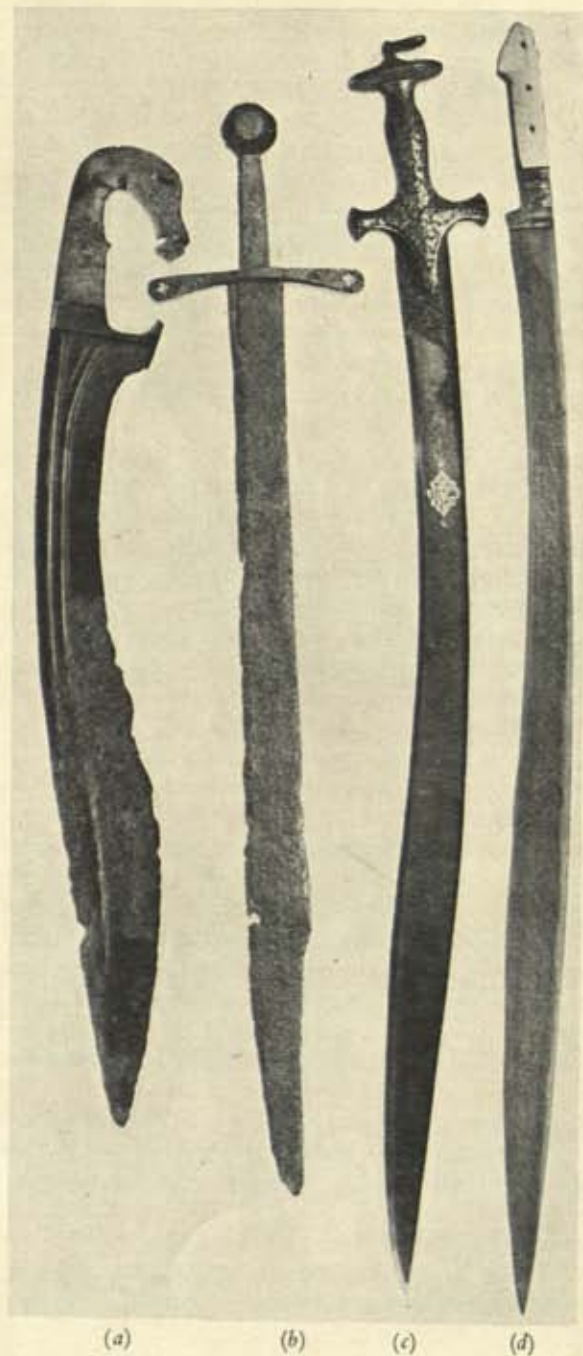


FIG. 3. A 'MACHAIRA,' FALCHION, SABRE AND YATAGHAN  
(a) Iberian machaira (British Museum); (b) Falchion, English, fourteenth century (Norwich Castle Museum); (c) Indian sabre (Victoria and Albert Museum); (d) Yataghan (Victoria and Albert Museum)

The origin of the well-known Gurkha knife, the kukri, is untraceable. Its type is clearly identical with that we have been discussing. The blade is of *machaira* type but is more abruptly curved, so that the point normally plays no part in its use as a weapon. Like the *κόπρις* it is a utility knife, used, in common with the dah, bolo and machete as a slashing knife for jungle clearance. All of these are moreover excellent weapons, and so no doubt was the *κόπρις*. Petrie states that the kukri was probably a legacy of the Bactrian Greeks,<sup>21</sup> but Sandars, who is very impressed by the elegance and efficiency of the Iberian sabre, is emphatic that there is no connexion whatsoever between the *κόπρις* and the kukri, rejecting for the latter any claims to ancient origin. There is not the slightest evidence to support either of these two contentions, but though derivation from the Bactrian Greeks cannot be proved, an early appearance of sabres of this type in India can be clearly demonstrated.

A *machaira*-like sword was in use in India not later than the seventh century A.D. when it appears in the hand of a *yogini* in the well-known relief at Mamallapuram showing Durga attacking the Buffalo Demon, and again wielded by a foot soldier in one of the battles depicted in the rock paintings of the Mahadeo Hills, perhaps as early as the fifth century (fig. 2k, l). The example from Mamallapuram has a typical Indian hilt and so has that on fig. 3c, which has that normal to the Indian tulwar. The yataghan shown here (fig. 3d) is of the kind associated with Persia, Turkestan and the Caucasus, though its actual provenance is Nepal. It has a modified form of Turkish yataghan hilt, which has as a rule a pommel with wide flaring wings. This form of hilt, described by Schaeffer as 'en forme de deux éventails accolés,'<sup>22</sup> is most interesting as it would appear with little doubt to derive from that which was prevalent during the early Iron Age in north-west Persia (Talish) and the Caucasus about 1000 B.C., a good example being from Ardabil now in the British Museum, a straight cut-and-thrust sword 29.9 inches long (fig. 2e-h).

Setting aside the clearly worthless expressions *sichel-schwert* and *sickle-sword*, what translations or equivalents have we for ancient or foreign words such as *khopesh*, *machaira* and yataghan, always provided that they are considered necessary? The *khopesh* and similar weapons are classed as scimitars, and this is a reasonable rendering giving an impression of curved oriental swords, which in fact they are. As regards the word scimitar, there are at least fifteen modes of spelling recorded, including in 1548 'girded with two swords called cimiteries'; but still earlier in 1540, in Chr. Richerius Thorigneus's *De rebus Turcarum*, 'cymitharra' is given as the name by which the Janissaries called one of their weapons.

Most of the ancient authors who mention the μάχαιρα are writing in Greek, but it is clear that the Romans knew of this sword and called it *machæra*. Recent writers in English have called it a sabre, a term preferable to the Spanish '*espada falcada*' which seems to echo the *falcatus ensis* of Ovid, apparently a unique literary expression thought up by himself.<sup>23</sup> For though Sandars uses '*falcæ*' to denote (a) an object carried by a horseman on an Iberian



coin, so cramped by the space available as to be indistinguishable but probably a whip, (b) the hooked sword in the hand of the warrior on the Iconium stele, possibly the *drepane*, but perhaps shown by the sculptor in the reverse position of its actual use, (c) the sword of a Persian shown on a late alabastron, where it almost certainly represents a scimitar with the sharp edge on the convex side, there is no evidence to show that this word was ever used to denote a weapon.

In spite of the contrary testimony of the monuments, we must accept Xenophon's classification of the *machaira* as a cavalry weapon; after all he had more first-hand contemporary experience than the sculptors or vase painters. That the average *machaira*, be it Greek or Iberian, was about 23 inches in length would not hinder it from being an effective cavalry weapon at a period when the horses available probably did not exceed 14 hands. Sabre, therefore, with its established cavalry connotations, is as good a word as we are likely to find. Both scimitar and sabre seem to be words of obscure origin, but there is little doubt that they derive from the East and that their use in Europe is late.

The oriental khanjar, a dagger somewhat resembling the yataghan, which Sidney Smith associates with the *khopesh*,<sup>24</sup> is connected with a characteristic etymological tangle. Yule and Burnell in *Hobson-Jobson* regard hanger as a corruption of khanjar, and, in spite of contrary opinions expressed in various leading dictionaries, it is probable that they are right. The word handjar, also handiarre and haniar, is without doubt true Hobson-Jobson for khanjar, one of the essentials being the substitution of familiar syllables for unfamiliar ones. It appears in Knolles's *History of the Turks* in 1603, a date admittedly more than 100 years later than the first recorded mention of a hanger in the Howard Household Books of 1481-90. It does seem, however, that the word hanger was used in dialect for the sword-frog in which the scabbard hung,<sup>25</sup> a sensible use of the word which might have been transferred to the sword itself under the influence of such words as handjar and whinger.

It is doubtful whether the words whinger, quinger, whinyard, etc., have any connexion with khanjar. The suggestion that they are onomatopœic, imitative of the sound of a stroke, seems a good one. As an expression of sound, whing would be in line with many other sound words ending in -ng, including ding which also has the meaning of striking. It is at any rate clear that not only is the tracing of the nature and development of the weapons we have been studying complicated and confused by those that portrayed them, but even more so by those who have mentioned or described them in their writings. As is always the case, much more remains to be discovered about these early weapons which we may continue to think of as scimitars and sabres. These notes may, however, serve to put any future study on a firmer basis by the clarification of the typological features held in common, by the elimination of the non-existent sickle-sword together

with such descriptions as *harpé* and various inappropriate derivations from *falx* and by the indication of the probable place of principal development, if not of origin, in Asia Minor and Syria.

## Notes

<sup>1</sup> Petrie, *Tools and Weapons*, sections 65, 63. Brit. Sch. Archaeol. in Egypt, Vol. XXVIII, 1917.

<sup>2</sup> Maxwell-Hyslop, 'Daggers and Swords in Western Asia,' *Iraq*, Vol. VIII, 1946, p. 41.

<sup>3</sup> This error features prominently in 'Note concerning the distribution of the Sickle-Sword' by P. Lenk-Chevitch, *MAN*, 1941, 60.

<sup>4</sup> *Nouvelles Fouilles de Tello*, 1910, pp. 128f. and Plate VIII, 4, 5. (Note: all line drawings, with the exception of fig. 2k-n, are drawn to the scales indicated as closely as available information permits.)

<sup>5</sup> Sichern: Fr. Bohl, *Die Geschichte der Stadt Sichern*, 1926, Plate V. Byblos: Virolleaud, *Syria*, Vol. III, 1922, Plate LXV, and Maxwell-Hyslop *loc. cit.* Plate IV, 34.

<sup>6</sup> Dörpfeld, *Troja und Ilion*, Vol. I, fig. 413, calls this object by the non-committal name of 'gerät.'

<sup>7</sup> Gezer: Macalister, *Excavations of Gezer*, Vol. III, Plate LXXV, 16, 19. Phœnicia: Dussaud, *Syria*, Vol. VII, 1926, fig. 1 (shows a number of known examples). Ras Shamra: Schaeffer, *Syria*, Vol. XVII, 1937, Plate XVIII.

<sup>8</sup> Albright, *The Archaeology of Palestine*, Pelican Books, 1949, fig. 31 (man behind chariot).

<sup>9</sup> Schaeffer, *Stratigraphie Comparée et Chronologie de l'Asie Occidentale*, p. 197.

<sup>10</sup> Seton Lloyd, *Early Anatolia*, Pelican Books, 1956, Plate XIV, a. (There are very many earlier reproductions, but this is very clear and easily obtained.)

<sup>11</sup> J. L. Myres, *Who were the Greeks?* 1930, p. 590, note 109.

<sup>12</sup> Pierson Dixon, *The Iberians of Spain*, p. 58, Plate VI, b.

<sup>13</sup> S. Casson, *Macedonia, Thrace and Illyria*, pp. 303f.

<sup>14</sup> A full account of these sabres with a valuable dissertation on the *kopis* and *machaira* is given by H. Sandars in 'The Weapons of the Iberians,' *Archæologia*, Vol. LXIV, 1912-13, pp. 205-94, and also published separately.

<sup>15</sup> Traubingen: Déchelette, *Manuel d'archéologie*, Vol. II, Part 2, p. 282, and Petrie *loc. cit.*, Plate XXIV, 34. Franconia: Petrie, *ibid.*, Plate XXVI, 149.

<sup>16</sup> Petrie, *loc. cit.*, Plate XXXII, 34, 38. Information from Mrs. Maxwell-Hyslop in correspondence suggests that these similar weapons are not necessarily of one period. The figure on the Etruscan stele of Larthe Atharnie, datable to the seventh-sixth centuries B.C., carries a sword of this type, but the example from Tolentino is a typical *machaira*, unlikely to be earlier than 500 B.C., and a similar blade is shown on one of the pillars of the late 'Tomb of Reliefs' at Caere.

<sup>17</sup> Many artists use a falchion style of sword where orientals are concerned. Botticelli depicts it a number of times, notably carried by Judith and also by Mercury in the *Primavera*.

<sup>18</sup> Perrot and Chipiez, *History of Art in Sardinia, Judæa, Syria and Asia Minor*, 1890, fig. 359. It was discovered and copied by Texier about 1836; it is probably of the fifth century B.C.

<sup>19</sup> Von Cles-Reden, *The Buried People*, 1955, Plate LXX.

<sup>20</sup> This sword from Hyderabad State is part of the Kitchener Collection on loan to the Victoria and Albert Museum.

<sup>21</sup> Petrie, *loc. cit.*, p. 28.

<sup>22</sup> Schaeffer, *Stratigraphie Comparée*, p. 437.

<sup>23</sup> This use of *falcatus* with *ensis* in *Metamorphoses* I, 717, seems to be the only instance recorded in any dictionary.

<sup>24</sup> Sidney Smith, *Early History of Assyria*, p. 137.

<sup>25</sup> In J. O. Halliwell's *Dictionary of Archaic and Provincial Words*, Vol. I, 1846, a hanger is described as 'the fringed loop or strap appended to the girdle, in which the dagger or short sword hung; "Men swords in hangers hang fast by their sides," Taylor's *Workes* 1630.'



## OBITUARY

## Osbert Guy Stanhope Crawford: 1886-1957

15 The death of O. G. S. Crawford on 28 November, 1957, at the age of 71 has removed another of the great figures in British archaeology, and his place will not easily be filled. Not that he was simply a field archaeologist who had the gift of making all that he wrote interesting as well as stimulating; he was also by inclination and training a geographer, with a strong anthropological approach to his work. He had also an interest in the people as well as the things they left behind them, and advocated attention to the common things of daily life 'because they are common . . . and because they are not kept.' His plea for fuller description of how people live, build their houses, and make their pots, in addition to the description and analysis of their social systems, is worthy of note by future anthropologists. This aspect is well brought out all through his *Archaeology in the Field* (1953). His work with Sir Henry Wellcome in the Sudan in 1914 led to an interest in the archaeology and topography of that region, which eventually produced *The Fung Kingdom of Sennar* (1951), a storehouse of information to which (lamentably) he failed to provide an index. This study gave rise to an interest in Ethiopian topography, and his last book, *Ethiopian Itineraries circa 1400-1524* (written for the Hakluyt Society), went to press the day he died. But his greatest and probably most enduring monument is *Antiquity*, known to all archaeologists, and I hope also to most ethnographers. In addition to this, he was one of the pioneers of air photography in archaeology, and used air photographs to a considerable extent in both *Antiquity* and his own books. Yet another side of his activity was his work as Archaeology Officer at the Ordnance Survey which had indirect

results in many other ways than mere correction of map entries.

Though not latterly a Fellow of the Institute, Crawford was first elected in 1911, and was re-elected in 1922 after war service from 1914 to 1918, during which he was a prisoner of war and made a successful escape from his camp but was recaptured.

Crawford was not academically minded, but he valued very highly the Litt. D. which Cambridge gave him in 1952. In *Archaeology in the Field* he relates how, to demonstrate some fundamental point, Haverfield once exhibited a single potsherd to a shocked audience of Antiquaries; Crawford himself might have done such a thing—indeed I have little doubt that he did similar things. He was a man who wrote and said what he believed to be reasonable and true, but without malice; and his criticism was usually just. I think that his general attitude is well summarized on the last page of his autobiography *Said and Done* (1955), where he wrote that *Antiquity* was open to anyone 'who has the right ideas about archaeology.'

He liked many things: the desert, because one can get away from people there (though he evidently never got lost in the 'bush'); he also liked cats, and not very long ago gave a characteristically Crawfordian broadcast on the language of cats—his own cats, of course.

The destruction in the blitz of 1940 of the materials for a topographical study of Wessex archaeology has deprived us of what might have been his most important book, for it was when writing on such subjects—Saxon charters, linear ditches, mediæval forests, and the like—that Crawford was at his best and most interesting.

G. W. B. HUNTINGFORD

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

Some Aspects of Samoan Material Culture. By G. B. Milner, School of Oriental and African Studies, University of London. Summary of a communication to the Institute, 21 November 1957

16 Mr. Milner showed three colour films made while he was gathering materials for a new dictionary of the Samoan language. He said that in his opinion it was essential for a lexicographer to gather not only words, but words in citations and citations in their proper contexts. In order the better to do so, he had found it necessary to do his work in two Samoan villages. In such an environment it was less difficult, given a certain amount of first-hand participation, to grasp the meaning of isolated words in specialized activities. To give a proper translation for a given word, thus bridging the gap between two cultures as widely separated as those of England and Samoa, was not necessarily a matter to be settled immediately, provided that one had, while in the field, adequate means of storing and preserving information,

and, later, access to specialized technical opinion. To illustrate one method of storing information in a visual form with a view to correlating it subsequently with its linguistic aspect, he showed in the form of cinematographic abstracts a number of activities involving technical terms, both in Samoan and in English. These were ancient Samoan team games, the making of household articles from the leaf of the coconut palm, certain aspects of house-construction, the weaving of mats and the preparation of bark-cloth.

In the discussion which followed Mr. A. Digby of the British Museum underlined the need for a complete pictorial record of a given technical process including if possible information as to the time taken by each operation. Professor Raymond Firth, in the Chair, while agreeing with the speaker made a clear distinction between the record of actual events and the record of technical processes demonstrated to a field investigator, each being likely to have its own time scale.

## SHORTER NOTES

### Excavations at Ife, Nigeria

17 The excavations referred to in the legend to Plate A in the January issue of MAN (describing the discovery of bronze figures at Ife in Yorubaland in late November) have been carried on during the months of December and January by Mr. Frank Willett of the Manchester Museum, who flew to Nigeria at a week's notice at the instance

of the Director of Antiquities, Mr. Bernard Fagg. The bronzes, discovered by a builder's workman at Ita Yemoo on the outskirts of Ife, appear to be of the same style as the heads similarly discovered at Wunmonije in 1938 (*cf.* MAN, 1938, 201), which have been provisionally attributed to the thirteenth-fourteenth centuries. It is now learned that a large number of fragmentary terra-cotta sculptures have come to light in and near the site and also



a potsherd pavement. The Hon. Editor of MAN hopes to publish further news of the excavation as soon as it becomes available.

### A Register of Archaeological Field Research in Progress and in Plan

**18** A little more than a year ago the Hon. Editor of MAN announced (1957, 6) that he would be glad to receive, for publication, summary information about current or projected field expeditions which are wholly or partly concerned with anthropological research. While he now cordially renews that invitation to all persons and institutions concerned in such projects, he recognizes that their initiative will need to be supplemented by purposive and systematic collection of information, if MAN is to offer its readers a reasonably comprehensive picture of what is being done in the world, or indeed in any area or subject. He therefore hopes to find a number of helpers who will undertake the task of keeping in touch with anthropological field research of the kinds with which they are conversant and of compiling the information for publication. As was made clear in last year's announcement, by anthropological research is meant research in any subject which is within MAN's purview, and this includes most branches of archaeology, physical anthropology (with serology), social and other cultural anthropology and ethnology, folklore, etc. It is better to include too much than too little, since one of the main functions of MAN is to keep anthropologists in touch with neighbouring sciences.

For the archaeological field, as a start, the Hon. Editor has been fortunate in enlisting the services of Mr. G. de G. Sieveking, Assistant Keeper in the Department of British and Mediaeval Antiquities of the British Museum (London, W.C.1), and Hon. Assistant Editor of the *Journal of the Royal Anthropological Institute*,

as a collector of information, and it is proposed to publish such information in MAN in the form of a Register of Archaeological Field Research in Progress and in Plan, probably at annual or semi-annual intervals. Mr. Sieveking will be pleased to receive information in any form, and will himself write to appropriate persons in all parts of the world. It is hoped to include in each case the name of the site where excavation is to take place, or is taking place, its geographical position, the periods or dates which it is believed to cover, and the name and address of the director of the excavation and those of the institution, if any, to which he is attached. Continuations of excavations begun in the preceding year or earlier will be included, especially as such continuing research is often the most important.

### Horniman Museum Lectures, Spring, 1958

**19** The following are among the lectures of anthropological interest to be given at the Horniman Museum, London, S.E.23, at 3.30 p.m. on Saturdays: 8 February, 'Puppets of Java, Bali and Malaya,' by Mrs. J. M. F. Jones; 8 March, 'The Music of South India,' with recordings, by Mr. J. R. Marr.

### Acknowledgment of Grants towards Publication in MAN

**20** The Hon. Editor of MAN acknowledges with gratitude the receipt of financial contributions towards the cost of articles of unusual length or number of illustrations: from Imperial Chemical Industries, Ltd. (Metals Division) for Professor F. C. Thompson's 'The Early Metallurgy of Copper and Bronze' (1958, 1); and from the Sarawak Government for Mr. T. Harrisson's 'The Great Cave of Niah' (1957, 211).

## CORRESPONDENCE

### Joking Relationships in Africa. Cf. MAN, 1957, 140, 225

**21** SIR,—It has recently been suggested by Professor M. Wilson, concerning the Nyakusa, and by Dr. P. H. Gulliver, concerning the Songea Ngoni, that the custom of *utani* or *ubutani* between these tribes is a comparatively new phenomenon, connected with modern economic conditions of labour migration, which force tribes with a tradition of rivalry and hostility to mix amicably. In both cases the data supporting this hypothesis were obtained from the tribesmen themselves, and in both cases we are told that the inter-tribe joking relationship has been modelled on the lines of a traditional joking relationship between cross-cousins.

There is evidence to show that the custom of inter-tribe *utani* was established further north in Tanganyika before the time of mass labour migration. Dale (*J. Anthropol. Inst.*, Vol. XXV, 1896, p. 237) writing of the Bondei, mentions that a man and his *mtani* might seize from each other small items of property, and describes certain other functions of the *mtani*. He also says that 'each tribe is the "mtani" of another tribe and that tribe of it,' but does not specify the tribes concerned. Moreau (*Tanganyika N. & R.*, Vol. XII, 1941, pp. 1-10), who refers to this description, justly describes it as 'unsystematic and incomplete'; he himself says that 'some Bondei recognize rights of mutual hospitality, which are perhaps vestigial, with certain of the Digo' (*Africa*, Vol. XIV, 1943, pp. 386-400). He seems to have missed a slightly more detailed reference to the custom of *utani*. Krelle, writing in 1907 or shortly afterwards, mentions that the custom of *utani* (Swahili term) or *ugongo* (Saramo term) exists between certain clans in the Saramo tribe, and between the Saramo and Nyamwezi tribes. He describes it as a sort of joint property ownership, in which a man might take from his *mgongo* anything he fancied, such as chickens, ducks and maize, without either asking or being punished. Klamroth, in the

same paper, describing the Saramo, says that the *utani* relationship between tribes is supposed to have existed in the past; he too describes Saramo inter-clan *utani*, mentioning that the *wangongo* or *watani* bury each other's dead (*Das Eingeborenen Recht: Ostafrika*, edited by E. Schulz-Ewerth and L. Adam, Stuttgart, 1929, preface, pp. 237, 12).

This Saramo-Nyamwezi joking relationship is, I imagine, the one referred to by Moreau (1943), who prefers the spelling 'Zaramu.'

The territories of the Nyamwezi and the Zaramu are several hundred miles apart, and we may enquire under what conditions a joking relationship between these tribes might function. Moreau (1941 and 1943) also names certain other tribes having a joking relationship with the Nyamwezi, and points out that 'the tribes named are those on the trade routes from Tabora to the coast, along which the Nyamwezi, as the pre-eminent porter tribe, were constantly passing.' Marsh and Kingsnorth (*An Introduction to the History of East Africa*, 1957) say that Tabora was founded in or around 1830, and soon became the main centre of communications and trade (mainly in slaves and ivory) in the East African interior, the equivalent of Zanzibar on the coast. Speke (*Journal of the Discovery of the Source of the Nile*) suggests that native tribes fought each other to get slaves for trade with the Arabs, and the reputedly warlike Nyamwezi may well have indulged in such trading. F. Spelling ('Die Wanjamuesi,' *Zeitschr. f. Ethnol.*, Vol. LIX, 1927, pp. 201-52) says that the Arabs recognized the Nyamwezi as a powerful force in East Africa and made use of them. Portage, especially of ivory, continued after the decline of slaving. Moreau says that the Nyamwezi probably did not raid down to the coast, and this seems very likely in view of the role they played in trading and the importance for them of an open route to the coast.

It is unlikely that there was any large-scale labour migration from the Nyamwezi to the coastal work areas in the first decade of this



century. The Dar es Salaam-Tabora railway, although started in 1905, did not reach Tabora until 1912. However, that there was some foot travel over this colossal distance is clear. Spelling mentions that he knew some Nyamwezi who had made the journey to the coastal plantations on foot 20 or more times. Such men are likely to have been travelling in the first decade of the century. He also says that one of the most noteworthy features of the Nyamwezi, distinguishing them from all other East African tribes, is their 'Wanderlust.' Add to this such statements as that by Steere, writing of the Zaramo in 1869, that 'the main road to the Nyamwezi lies for a long way through their [i.e. Zaramo] possessions' (Steere, *Short Specimens of the Vocabularies of Three Unpublished African Languages*, 1869), and the conditions underlying the joking relationship between the Nyamwezi and the Zaramo begin to take shape. The existence of this joking relationship contributed to the maintenance of friendly relations between tribes which crossed each other's territories.

With regard to the theory of Professor Wilson and Dr. Gulliver, may not the present-day joking relationship between the Nyakusa and the Ngoni be a continuation under modern conditions of a traditional joking relationship between these two tribes, as appears to be the case in the Nyamwezi-Zaramo joking relationship? The statements made by the natives could then be considered as attempts to explain an institution in the most logical way they could. I am at present unaware of any historical data which can settle this point.

University College, London

VERNON REYNOLDS

**The Art, Myth and Symbolism of Arnhem Land.** Cf. MAN, 1957, 241; 1958, 5

22 SIR.—Dr. Worsley's review of Mr. C. P. Mountford's *Arnhem Land: Art, Myth and Symbolism* raises the point whether the recording, without comment or analysis, of aspects of culture which are bound to disappear before long is a useful activity. I hold that it is. Dr. Worsley does not. There has hitherto been no such extensive and copiously illustrated account of the art and mythology of Arnhem Land, and the correlation of the two adds greatly to its value. Certainly this would have been increased if art

and mythology could have been set in their social context, but the fact that this was not possible does not mean that the book is useless. If a thorough investigation of Arnhem Land social structure is made before it is too late many anthropologists may be glad that Mr. Mountford has preserved this corpus of related information, for already, I gather, in some districts a good proportion of the bark paintings are made to order for the mission stations.

The review seems to have been written with the reviewer's personal interests too exclusively in mind. He may be right in saying that this book can only be of limited value to the social anthropologist, but when he speaks also for the artist, the student of folklore and the comparative ethnologist almost anyone is entitled to disagree. Still more sweeping is his condemnation of the whole purpose of the expedition, most of the members of which were natural scientists, on the basis of the first volume of the reports covering only one aspect of one subject. It would be interesting, incidentally, to know what sort of errors are so numerous: as far as accuracy of reproduction is concerned the text figures seem to set a particularly high standard.

British Museum, London, W.C.1

B. A. L. CRANSTONE

**Rock Gongs and Rock Slides.** Cf. MAN, 1956, 23, 73; 1957, 32-34, 96, 142, 182, 250, 251

23

SIR.—In MAN, 1957, 34, a letter signed by Miss M. A. Bennet-Clark mentions a rock slide on the south-eastern slope of the Hill of the Nymphs in Athens.

I have just been on the spot and have carefully looked for the slide in the place where the *Guide Bleu*, 1950 edition, indicates that it is to be found.

I can safely say that no rock slide is there any longer today. Very probably a small house with a hen yard now occupies the site. From information gathered from local inhabitants of the neighbourhood, the rock slide has ceased to exist for quite some time now.

All that is left to barren women seeking to remedy their sterility is praying in the church of Haghia Marina which crowns the hill.

Athens

PETER, Prince of Greece and Denmark

## REVIEWS

### GENERAL

**Man and Culture: An Evaluation of the Work of Bronislaw Malinowski.** Edited by Raymond Firth. London (Routledge & Kegan Paul), 1957. Pp. viii, 292. Price £1 12s.

24 No one who was professionally active in anthropology between 1920 and 1940 can read this book without nostalgia, so vividly does it bring back the disputations in which Malinowski was such a challenging figure. One recalls, with a sharpness that two additional decades cannot dim, the stimulating impact of what I feel to have been his best book, *The Argonauts*. And one remembers, too, with more tolerance and humour than was possible at short range, this *enfant terrible* of our science, with his insistence that Functionalism was the only true anthropological faith and that he was its prophet. All this, and more, comes out clearly in the evaluations of various aspects of Malinowski's work by some of those who studied with him and who, with the passage of time, have been able to bring to their analyses a balance and objectivity that makes of their joint effort a notable contribution to the literature of our science.

The Editor, Raymond Firth, opens the book with an introduction, giving its terms of reference, a short biographical sketch of Malinowski, particularly as regards his scientific activities, and the influences which shaped his theoretical and methodological position. Audrey I. Richards follows this with a discussion of the concept of culture as it figures in his work. Ralph Piddington analyses his theory of needs, and Talcott Parsons assesses the implications of his writings for the theory of social systems. From theory the discussion moves to methodology: Phyllis Kaberry on his approach to field work and the writing of ethnography and (two chapters later) E. R. Leach on the epistemological base of his position. A number of chapters are devoted to Malinowski's work in specialized fields: J. R. Firth on linguistics, I. Schapera on law, Meyer Fortes on kinship, S. F. Nadel

on magic and religion, and Raymond Firth on economics. Finally, there are considerations of his approach to the study of social change by Lucy Mair, and to what may be subsumed under the phrase applied anthropology, here called 'public service,' by H. Ian Hogbin.

Certain points about Malinowski come out with great clarity. Again and again we read that he was a great teacher, a fine fieldworker, a poor theoretician. It enters at the very outset of the book, where R. Firth poses the 'great question . . . why, if his theory was so inadequate, was his influence upon his pupils so profound?' (p. 2). Piddington, who defends Malinowski as a theorist more vigorously than any other contributor, speaks (on p. 43) of the fact that 'Malinowski was often inconsistent, even uncritical, in his selection and use of terms.' . . . So far as the general theory of social systems is concerned, concludes Parsons (p. 70), 'there is with one exception no point at which Malinowski has more to teach us than one or several of his contemporaries or predecessors' and this exception he 'entirely failed to follow up.' But, adds Parsons, 'the hardness of this verdict is mitigated,' for one thing, by 'the eminence of Malinowski's contributions to anthropology and more broadly to social science in other directions, as a fieldworker, a teacher, and a "clinical" theorist.' Leach, who points out the deficiencies in Malinowski's logical position, (p. 132) concludes his chapter on a lighter note by adding (p. 127) that 'Malinowski, I have insisted, was in bondage to his predecessors; he resented their existence because he was so much indebted to them. Some of us perhaps feel the same about Bronislaw Malinowski.' One can cite Schapera's assertion that 'Malinowski's exposition of his own views contains various instances of inconsistency and contradiction' (p. 146) and Fortes' opening sentence: 'It is impossible for anyone who was a pupil of Malinowski to write about his work quite impersonally' (p. 157). These examples could



be extended far beyond the permitted length of any review, to show how unanimous is the verdict of this group of gifted men and women who studied with him, many of whom, in their own right, have become outstanding figures in world anthropology.

As with most collaborative volumes, the contributions differ in manner of treatment, and are uneven in the incisiveness of their analyses. One must, however, note the clarity of Parsons's discussion, the freshness of Leach's approach, the insight of Schapera's presentation. Richards's critique of the use of research models (pp. 26f.), Kaberry's comments on the need for flexibility in planning and executing field studies (p. 88), or the statement of the possible objectives of an applied anthropology by Hogbin (p. 247) have implications that go far beyond the intent of this particular book. Some of the contributors were handicapped by the nature of their assignment. It could not have been easy for Mair to write on Malinowski's study of social change, when his position was ahistorical if not antihistorical, and the materials on which she could draw were afterthoughts, as it were, published towards the end of Malinowski's life, and among the least valuable of his works. Nadel inevitably had to throw his discussion of magic and religion out of balance by devoting far more attention to the former than the latter since, as he put it, 'one suspects that to the pragmatist Malinowski the subject matter of religion was less congenial than that of magic' (p. 205)—a suspicion easily confirmed by noting Malinowski's conspicuous blindness in his field researches to Trobriand religion.

The book labours under restrictions that arise out of its terms of reference. As implied in its Editor's introduction (p. 1) and as frequently indicated elsewhere, Malinowski's work is considered as it bears on 'modern British social anthropology.' This is perhaps why, for instance, J. R. Firth quite ignores the international influence which Malinowski's linguistic work, such as it was, had on the development of semantics. The very title of the book, *Man and Culture*, provides a framework that is evidently not too comfortable for the contributors who, with one or two exceptions, have for some time made the concept of culture incidental to their thinking, where they have not explicitly rejected its utility, and who have been far more interested in social structures than in man. Social anthropology having largely given over historical for analytical concerns, and having stressed the intensive study of single societies without recourse to comparison, the authors give the impression from time to time of being ill at ease with assignments that require a firm grasp of the history of anthropology, and that often call for documentation on the comparative level.

In terms of giving an evaluation of all aspects of the work of Malinowski, the inclusion of a chapter discussing his theory of myth, and through this his marked influence on contemporary literary criticism, would have been welcome. There are, indeed, 18 entries under 'myth' in the Index, and Nadel, perforce, discusses certain ideas which Malinowski held on myth in his treatment of magic. But can one, for example, really reach an adequate understanding of the important point of Malinowski's approach towards 'savages' without referring to his thesis regarding the lack of creativity in their narrative forms? Or, for instance, does not the impact of his concept of myth as a 'charter of belief,' so widely discussed by anthropologists concerned with the role of myth in human life and thought, call for more than the passing mention given to this contribution to our science? A chapter could well have been assigned to Malinowski's psychological orientation. The importance of psychology in Malinowski's formulations is reflected in the mention made of it throughout the book. Fortes comes nearest to a sustained analysis of its significance when he analyses with great acuteness Malinowski's use of the Oedipal relationships and its relevance for the study of social structures. But this is only one phase of the topic. I hold, in principle, that a book should be assessed only in terms of its own frame of reference. Yet in this case, where that frame clearly excludes critical aspects, I cannot but express my regret that the resulting lacunae were left unfilled.

The book clearly demonstrates that Malinowski was no social anthropologist. Culture was his primary term of orientation, and for all his use of the concept of the institution, he was essentially a humanist. This fact takes on particular significance in the recent history of anthropology in Britain, because of his relations with

Radcliffe-Brown. These two figures dominated the field in the country for almost the whole period between the two World Wars. And though both began as functionalists, and, so to speak, comrades in arms, Brown soon left this keyword to Malinowski, and turned to social structure as his core formulation. It was, in many aspects of the situation at the time, a struggle for power, and in this struggle Radcliffe-Brown won. His ghost stalks the pages of this book; 11 titles from his slender bibliography of published works are cited; there are 45 entries in the Index under his name. And though it is Malinowski's contributions on which the discussions focus, discussions by men and women some of whom studied with both men, the essential conceptual and theoretical points of view of almost all the contributors are those of Brown, rather than those of Malinowski.

It will be apparent how stimulating this book is. I predict that for many years it will have a prominent place in the reading that we will require of our students in teaching them about this phase of the development of anthropological science.

MELVILLE J. HERSKOVITS

**The Great Mother: An Analysis of the Archetype.** By Erich Neumann, trans. from the German by Ralph Manheim. London (Routledge & Kegan Paul), 1955. Pp. xliii, 380, 185 plates, 74 figs. Price £2 10s.

25

As the author truly says, 'it is very hard for those who have not experienced the reality of the archetype by undergoing analysis to understand what depth psychology means by an archetype.' He makes comprehension as easy as he can by dividing his book into two parts and suggesting that some readers may prefer to omit altogether Part I, which is concerned with structural analysis. Those not expert in psychology might in fact be well advised to read the introduction to Part II first. Only a psychologist could pronounce on the theoretical merits of such a book. An anthropologist can but discuss that part of the argument of which he has some knowledge and comment on certain aspects of the method.

Dr. Neumann builds his thesis on a study of cultures of all levels, in all continents and of all periods from the European Palaeolithic onwards. The dust-cover, inevitably, mentions *The Golden Bough*, but the comparison is unfortunate, for though Dr. Neumann covers a vast field he has not, in regard to extinct and exotic cultures, read sufficiently deeply or critically. He relies heavily on Bachofen and Briffault, but though he refers to 'the sociological school' he is clearly unfamiliar with the work of modern social anthropologists. He launches into a discussion of 'The Symbolism of the Terrible Mother in Melanesia' apparently knowing only Layard's *Stone Men of Malekula*; neither in the footnotes nor in the bibliography does he cite (considering the New Hebrides only) Codrington, Rivers, Speiser or Deacon, to mention the more obvious. Negative evidence can not be ignored. A wider knowledge of anthropology would have enabled him to avoid many half-truths and inadequately based generalizations. For example, it is 'a law of the primitive world view' that 'certain cosmic bodies, directions, constellations, gods, demons' are correlated with 'the zones and organs of the body' (p. 41); Jesus is a vegetation god, and the fact that he lay in a wooden manger is significant in this regard (p. 243; where is the evidence that the manger was made of wood?); the division of labour among 'early mankind' is always 'archetypally conditioned'—'there is no such thing as a sexual fitness or unfitness for this or that task' (p. 261); he still believes that sexual promiscuity was characteristic of early societies (p. 269); agriculture arose from the storing of tubers or grain by women (p. 284). He tries to evade criticism by pleading that he is concerned with 'psychohistory,' i.e. that 'primordial' and 'matriarchal' refer to early stages in the development of human consciousness. This seems like a plea to be allowed to ignore ascertained facts and probabilities.

Illustration is lavish. The quality of many of the plates is first class, but for others and for the text figures there is heavy reliance on drawings; it is notorious that even careful drawings are not to be trusted, especially when it is some detail that is significant. In some cases the points made by the illustrations seem invalid without the eye of faith. This is particularly true of the bronze male Benin heads which appear in the section illustrating aspects of the Primordial Goddess.



Depth psychologists seem rather given to adventuring into other fields without equipping themselves adequately (cf. MAN, 1952, 135). In this case the theoretical basis may be sound but its development does not inspire confidence.

The book is a translation, but few of its defects appear to be due to the translator, who seems to have done his work well. The price is remarkably low.

B. A. L. CRANSTONE

**Piecing Together the Past: The Interpretation of Archaeological Data.** By V. Gordon Childe. London (Routledge & Kegan Paul), 1956. Pp. 176. Price 18s.

26 Professor Childe gave his book a popular-looking title, and wrote it in a lively and interesting style. This should not disguise the fact that it is a serious study of the interpretation of archaeological evidence, treated in a readable manner in spite of a certain amount of jargon. It is based on a course of regular lectures at the Institute of Archaeology.

Professor Childe discusses the assumptions implicit in the interpretation of data provided by the field archaeologist, and does not ignore the difficulties of getting at the ideas which lie behind the purely material facts of archaeology. Nor does he hide the anomalies of systems of classification, and the difficulties which arise from a confusion of cultural and chronological terms. Frequent reference is made to the value of ethnological parallels in explaining archaeological data, and a valuable caution is given about injudicious use of this type of evidence. He draws attention to the fact that Marx did not declare that material culture caused spiritual culture, but that it determined it, i.e. that the progress of non-material culture presupposes progress in material culture. Unfortunately Professor Childe does not explain this statement or develop it. Continuing his critical survey, he discusses archaeological methods of dating and their limitations. He also puts the case for an increasing use of statistics for distinguishing stages in the cultural succession.

Professor Childe adds his support to the idea of classifying the Lower and Middle Palaeolithic together as the 'Proto-' or 'Archaeolithic,' and the Upper Palaeolithic and Mesolithic as 'Mio-' or 'Lepto-lithic.' He then discusses the problem of defining the Neolithic in such a way that it can be recognized in the field. In discussing the Bronze Age, he declares that those who have declared Beakers to be Neolithic have 'misunderstood what archaeologists are classifying and labelling' (p. 91). He discusses critically the various systems of naming cultures or periods, the distinguishing marks of cultures, their distribution and mutual relationships, and the problems of invention and diffusion. He puts forward on pp. 126, 127 new uses in archaeology for the terms 'class,' 'species,' 'genus' and 'family,' using these words in a way which is completely at variance with their centuries-old usage in systematics, and which must therefore be rejected as positively harmful.

This then, is an excellent book which really gets down to the fundamental problems of archaeology. It is full of stimulating remarks and though not all the suggestions in it will gain acceptance, the book merits the serious study of all archaeologists. FRANK WILLETT

**Charles Darwin.** By Ruth Moore. London (Hutchinson), 1957. Pp. 207. Price 12s. 6d.

27 The book begins with a chapter on 'the eminently curious Galápagos,' and the effect which the fauna of these islands had upon Darwin's mind. It goes on to a straightforward narrative of his life and works, including some account of the controversy aroused by the publication of *The Origin of Species*. His less familiar botanical researches are described, and his amusement noted at being elected as a botanist to the French Institute.

The book is very readable and attests the writer's competence in the natural sciences. Outside them her touch is less sure; the emperor Akbar appears as 'Abker' and we are told that the ancient Greeks made statues of Laocoon and 'Arretino.' RAGLAN

## AMERICA

**Tupari: Unter Indios Im Urwald Brasiliens.** By Franz Caspar. Braunschweig (Vieweg), 1952. Pp. 217, 36 illus. Price DM. 10.80. **Tupari (English Edition).** By Franz Caspar. Translated by Eric Northcott. London (Bell), 1956. Pp. viii, 224, 19 plates. Price 18s. 6d.

This is in no sense a systematic or analytical ethnological textbook. The more technological data collected by the author are reserved for a monograph to be published with the aid of the 'Swiss National Foundation for the Advancement of Scientific Research.'

We are given a straightforward but graphic narrative, told in an easy readable style, of the author's journey to the Tupari tribe (which occupies about one third of the volume) and his day to day experiences during some 6 months' residence in intimate association with them. The Tupari, who have never been properly described hitherto, live in the forest of the Matto Grosso, 8 days' march from the nearest outpost of a rubber ranch on the Rio Branco, a tributary of the Guapore river on the Brazil-Bolivia frontier. Their contacts with white men are slight and sporadic, and seem hardly to have influenced them at all except in reducing their numbers by disease (especially the common cold) to about 200 souls, or 40 families. The author took his courage in both hands (the Tupari being reputed to be cannibals), and decided to 'go native' without any white companions. He was given a hammock in the larger of the two communal domed huts or *malocas*, which housed the whole tribe. This hut was occupied by 30 families including the chief, and he was thus able to observe and record every detail of their daily and nightly life at close quarters. The author was, fortunately perhaps, not permitted to carry out his original intention of squatting as a mere observer. It was soon made clear to him that he was expected to take his full share of tribal life, and was compelled, rather against his will at first, to accompany the men to the forest and help in clearing and planting operations, hunting, fishing, and other activities such as *chicha* drinking feasts. His narrative naturally gains in stature and significance through these personal experiences, and

one is enabled to capture the flavour of tribal life in a most convincing way. He seems to have been accepted almost as a full member of the community; indeed the chief eventually pressed him to marry his daughter, and it was with some difficulty that he succeeded in rejecting the offer and making his departure. The chief's consent was only given on the strict understanding that he would return after visiting his relations at home.

One of the most interesting and original sections of the book is an account, illustrated by photographs, of a magical séance, in which sorcerers communicated with the spirits of the dead. The author acquired sufficient knowledge of the language to question them about their supernatural beliefs, legends of creation, and the spirit world, on which he obtained much valuable information.

This book should appeal equally to the general reader and to the trained ethnologist, though the latter will look forward to some amplification in the more technical monograph.

The translation by Eric Northcott preserves the freshness of the original text, and the English edition contains some new and well chosen illustrations, although it omits some of those originally published. It is improved by being provided with chapter headings and a list of contents. H. J. BRAUNHOLTZ

**The Eagle, the Jaguar, and the Serpent: Indian Art of the Americas: North America: Alaska, Canada, the United States.** By Miguel Covarrubias. New York (Knopf), 1954. Pp. xviii, 314, xi. Price \$15

29 This volume is in two parts. The first, amounting to nearly half the book, is an introduction in three sections: Origins of the American Indians, The Basic Horizons for the History of Indian Art, and The Technique and Aesthetics of American Indian Art. All these treat of America as a whole, so it is rather surprising to find that the second part, which deals with the aboriginal art of all periods, is confined to the area north of the Rio Grande. Both title and sub-title suggest that the author intended to add at least one



more volume, since jaguars and serpents, if not eagles, are more conspicuous in the art of the other Americas than they are in that of the North. As it stands, the book is ill-balanced, and it is a disappointment that we do not have Covarrubias's comments on the art of his own country, which he knew so well.

The book is aimed, not at the specialist, but at the general reader, and therefore it is necessary to say that it is likely to deceive him. The first section on Origins, in particular, is largely an *apologia* for an extreme diffusionist view, based on the theories of Heine-Geldern, Gladwin, Rivet and others, which would derive a great deal of the artistic heritage of America straight from Eastern Asia across the Pacific. It is not by any means impossible that some such transfers occurred, but Covarrubias, like some of his masters, overstates his case. Parallel is piled upon parallel with deliberate disregard of time and distance, and all difficulties are ignored. The same thread runs right through the book. This is not the place to enter the controversy, and it must suffice to say that it is far from being settled.

The section on Basic Horizons is an outline of the chronological sequence in Mexico and Peru. The possibility of its application to North America is hinted at but not pressed, and since the cultures of that area do not fit very comfortably into this frame, the section is not really relevant to the book as it stands. The chapter on Technique and Aesthetics contains much of interest, but again it has an eye to the whole of America and not only to the north. It is heavily weighted by the author's special interests; there are less than four pages on pottery, about a page and a half on weaving, and a third of a page on basketry, whereas there are nearly six pages on jade carving, which give occasion for much discussion of Covarrubias's favourite hobby-horse of parallels between Middle America and China, with never a word about North America.

The second part treats North America area by area—the Arctic, the Northwest Coast, and so on. It will give the reader a good idea of its best artistic achievements, and will be read with pleasure. The fine series of photographs illustrates this part; all are useful, particularly the considerable number hitherto unpublished.

The book is beautifully produced, with good type, admirable drawings by the author, and twelve coloured plates of North American objects reproduced from his paintings. These are in his well known, somewhat luscious style, which is always pleasing although the colours may be somewhat idealized. Miguel Covarrubias is dead. Whatever its defects, this book which he has left us is a work of art.

G. H. S. BUSHNELL

**Pascual de Andagoya: Ein Mensch erlebt die Conquista.** By Hermann Trimborn. Abhandl. a. d. Gebiet d. Auslandskunde, Vol. LIX, Series B, Völkerkunde, Kulturgesch. u. Sprachen, Vol. XXXIII. Hamburg (Cram, De Gruyter & Co.), 1954. Pp. x, 315, illus. Price DM. 40

The figure of the Spanish Conquistador Pascual de Andagoya has for long been a subject of intensive research by Trimborn, the results of which have been published primarily in the three following studies: 'Pascual de Andagoya and the discovery of Peru,' *El Mexico Antiguo*, Vol. VII, 1949; Pascual de Andagoya as Ethno-

grapher and Missionary in the Cauca Valley,' *Anthropos*, Vol. XLVI, 1951 (both in German); 'An unpublished map of Pascual de Andagoya,' *Trabajos y Conferencias*, Vol. III, Madrid, 1953 (in Spanish).

Whereas in the above mentioned works the author only illuminates certain aspects of Andagoya's activity, in the first part of his new publication he pursues the subject comprehensively and chronologically in its human and historical meanings. Trimborn takes into full account Andagoya's special and manifold merits in the field of discovery and exploration of the north-west coast of South America, as well as in its administration under Pedro Arias de Avila in Panama, and as an independent governor of San Juan in Colombia; furthermore in the faithful missionizing of his native subjects, and in the generally reliable exposition of the essential ethnographical background of the regions through which he travelled, viz. Colombia and the Isthmus territories. In the second part Trimborn treats in condensed form the previously published writings of Andagoya himself, as well as the accounts of him given by particular historians. In this connexion he gives us documents, which, though already published, are not easily accessible in their Spanish context, and translates them for the first time into German. These documents include the first publication in Spanish and German of Andagoya's letter to the Emperor Charles V about his expedition from Panama, and the continuation of his journey and investigations as far as Cali, dating from 15 September, 1540 (MS. 19267 in the Madrid National Library).

We should also specially mention the commentaries on each of the five chapters of Part I with their precise literary references, and the reproduction of four geographical maps and seventeen plates, which serve to illustrate the above-mentioned material.

With his publication on Pascual de Andagoya, Trimborn offers us a treatise, whose special value consists not only in his successful attempt at a logical and connected survey of all the available material (confused as it often is), but also in the thorough analytical and critical interpretation, with the sources, which he supplies both in the text and the accompanying notes. Both the author and the University of Hamburg, which made this publication possible, deserve our gratitude for their great efforts.

KARIN HISSINK\*

\*Translated from the German by H. J. Braunscholtz

**Geister am Roroima: Indianer-Mythen, -Sagen und -Märchen aus Guayana.** Edited by J. Huppertz. Eisenach and Kassel (Roth), 1956. Pp. ii, 209, 1 map

These stories, which Professor Theodor Koch-Grünberg collected during his expedition in the Guayana territory of Venezuela and Northern Brazil in 1911-13, have some interest as indications of a naïve but lively intelligence; the theme of the moon's two homes, for instance, is neatly combined with a human social setting and an observation of two planets (p. 25). Apart from this it is not easy to say to what public this book is addressed: the content of the stories will scarcely interest the general reader, while the folklorist may suspect them to be over-edited.

E. ETTLINGER

## EUROPE

**The Prehistory of Eastern Europe: Part I, Mesolithic, Neolithic and Copper Age Cultures in Russia and the Baltic Area.** By Marija Gimbutas. *Amer. Sch. Prehist. Res.*, Peabody Mus., Harvard Univ., Bull. No. 20. Cambridge, Mass., 1956. Pp. ix, 241, 50 plates, 126 text figs. Price \$7.50

Certainly Dr. Gimbutas may be said to be in an extraordinarily good strategic position, working in the U.S.A. and being a Lithuanian with full linguistic command of the Baltic, Finnish and Russian archaeological literature. But Dr. Gimbutas also has proved to be able to use this strategic position in an extremely scholarly way, and her present book undoubtedly will prove to be of fundamental importance for archaeologists in the western hemisphere who are interested in Eastern European problems. The scope of the book, however, is so wide and the material and problems presented

so manifold that the book cannot possibly be justly reviewed within a limited space. The author's acquaintance with the immense, and only with difficulty accessible, archaeological literature is impressive, and her taxonomic system really brings order into this enormous material. Certainly one may make small objections to the treatment particularly of the marginal areas: thus what is today considered north-western Europe may, of course, in prehistoric times have belonged to the north-eastern sphere; parts of the Scandinavian peninsula obviously are much more connected with Finland, the Baltic areas and North Russia than with any western culture areas; and parts of North Sweden and Finnmark definitely belong to what is here termed the 'Sub-neolithic culture in the forest zone of north-eastern Europe.'

Dr. Gimbutas often—and very illuminatingly—uses maps of distribution in part marked with possible routes of migration and



diffusion, in part also with the limits of certain cultures. I would not myself be able to check these maps, but certainly the north-western limits of the 'Corded Pottery and Battle-Axe' culture are too narrowly drawn (fig. 126); as a result the whole of Norway falls outside the limit, although the intrusion of both the Danish 'enkeltgravskultur' and the Swedish boat-axe culture has long since been recognized and has more recently been subject to a comprehensive monograph by Mr. Erik Hirsch. In any case there have been four distinct colonies of 'battle-axe peoples,' one of them as far north as the district of Trøndelag. Yet, of course, these are minor objections, due to my hesitation in following Dr. Gimbutas in some of her hypotheses on migration. But apparently she has cleared up definitely the controversial problem of the genesis of the 'Boat-axe,' 'Fatjanovo' and related complexes, supposed to mark the way of the Indo-European migration into Europe. Their derivation from the Kurgan people somewhere between the Black and the Caspian Seas seems to be very convincingly laid out.

On the whole, however, Dr. Gimbutas demonstrates an admirable command of a vast and complex material, and her book certainly will prove to be one of the most important contributions to European archaeology during recent years.

GUTORM GJESSING

**Barclodiad y Gawres: The Excavation of a Megalithic Chamber Tomb in Anglesey.** By T. G. E. Powell and G. E. Daniel. *Liverpool Monogr. in Archaeol. and Oriental Stud.* Liverpool (U.P.), 1956. Pp. xiv, 80, plates, line drawings, maps. Price £1 12s. 6d.

In this monograph the authors describe their joint excavation of the chamber tomb of Barclodiad y Gawres, in Anglesey. The first half of the work is devoted to a detailed description of the digging, helped by a lavish series of half-tone illustrations and good line drawings. This section begins with an explanation of the geographical and geological position of the monument and of its early history and ends with a description of the important series of carvings on five of the stones in the chamber and passage. There are five appendices dealing exhaustively with the small finds, the cremations, a trial excavation at the neighbouring tumulus called Mynydd Bach, a description of the Calderstones, Liverpool and a note on the derivation and meaning of the name Barclodiad y Gawres. There is also an index.

The main part of the monograph is devoted to discussions of the structure and evidence for rituals found at the site, and of the fine new series of carvings. Barclodiad y Gawres is fitted carefully into its place in the series of tombs of this type in Western Europe; we see it as an outlying and a fairly early example of the Cruciform Passage Graves of the Boyne, complete with elaborate mural art.

The excavations of stone-built tombs are difficult to describe clearly, particularly their structural details. The authors have succeeded very well, perhaps because they refer constantly to a good selection of plans, sections and photographs. The latter, it is true, vary in quality; for the most part, however, they add considerably to the reader's understanding of the architecture of this megalithic structure.

So little is known of the ritual connected with burial in this type of Passage Grave that the discoveries at Barclodiad y Gawres, though meagre, are of much importance. The recess in the passage containing an upright stone pillar with an oyster shell offering at its foot, the remains of a hearth and ritual stew in the central chamber, confinement of burials, by cremation, to the side-chambers and the elaborate blocking of one of the latter—these details add greatly to what we knew before of burial rites in the Boyne Culture.

The mural art found pecked into the surfaces of five of the stones (at the inner end of the passage and on two side-chamber stones) is described in some detail and an attempt is made to assess its position in the mural and plaque-idol art of Iberia, Western France and Ireland. At Barclodiad y Gawres the range of motifs includes both anthropomorphic designs and a superbly executed group of spirals. The authors conclude that stylistically the mural art found in this tomb should belong to an early phase of the art of the Boyne Culture.

If criticism can be found, it is perhaps in the publication of an excavation report—albeit an elaborate one—on its own, in a country where archaeological literature is already notoriously diffuse. Could it not have appeared in a volume of *Archæologia*? This, however, is not an important point.

NICHOLAS THOMAS

**Two Studies of Kinship in London.** Edited by Raymond Firth. *London School of Economics Monographs on Social Anthropology*, No. 15. University of London (Athlone Press), 1956. Pp. 94. Price 13s. 6d.

**Family and Kinship in East London.** By Michael Young and Peter Willmott. London (Routledge & Kegan Paul), 1956. Pp. xix, 232. Price £1 5s.

Our sociologists have recently discovered the English family. It has been a most welcome event, for their works provide a long-needed corrective to the 'pathological series of studies of "abnormal" and "problem" families, all heavily loaded with moralistic judgments which reached their eminence in the recent Report of the Royal Commission on Marriage and Divorce' (to quote Professor Titmuss's Foreword to the second of these volumes). It is a sad comment on how little we know of our own society that this discovery has been so largely an accidental one. In 1947 members of an anthropological seminar at the London School of Economics found that their views as to what English kinship practices were differed considerably. There was no literature on the subject to which they could appeal, so they carried out their own enquiry in a South London dockside borough. In 1954 Dr. Michael Young and his associates at the Institute of Community Studies began an enquiry into the effects upon family life of rehousing in new estates. They were surprised to discover that in Bethnal Green (where the Institute has its headquarters) the wider family was very much alive, and decided to make this the main subject of their research.

Both studies show that in districts of stable working-class settlement kinship relations are more extensive and patterned than might be expected. Both point to the strength of the mother-daughter relationship, which is such that according to the L.S.E. writers the local kinship system 'may be termed matri-centred or matril.' The same investigators found that women had a greater total knowledge of the kinship network than did men, and they very frequently served as 'pivotal kin' linking families otherwise ill-informed about others' addresses and perhaps even surnames. The kinship system of South Borough showed a high degree of personal selectivity enabling individuals to invest certain relations with a high emotional content, 'to treat some kin on a basis of close relationship and to relegate others to social limbo, to summon up or lay down the value believed to inhere in kinship more or less at will.' The results of this first enquiry were not followed up until 1954 when Dr. P. Garigue undertook a study of the kinship relations of London residents of Italian origin. Whereas the earlier investigators had concluded 'to be able to treat kinship as an instrument of social expression is personally important in the South Borough system,' Dr. Garigue found that in Italian culture kinship is more of a 'formal tie implying rights and obligations.' This may be linked with the greater opportunity in Italian life for kinsfolk to give one another material aid; certainly, the economic activities of the Italianates in London reinforce their kinship structure, and *vice versa*. This second study does not, however, complement the former very well; a study of the kinship relations of London Jews might perhaps have provided a more illuminating comparison.

The Institute of Community Studies book reports an investigation on a much larger scale, carried out over three years with an adequate research staff and a greater attention to the methodological issues. Dr. Young and Mr. Willmott found that in Bethnal Green the development of extensive kinship networks is closely associated with the length of residence of the population. People have their relatives near them. On marriage, a couple is more likely to stay with the wife's than the husband's parents, to find a house nearer the wife's mother than the husband's, but in either event his kin are unlikely to be far away. Tenants of privately owned flats or



cottage dwellings 'speak for' their relatives with the rent collector or agent and get them accommodation close at hand. In this way villages have been created in metropolis, and a man asking for help in getting another home could write: 'I am not particular where you send me, the further the better. I do not mind if it is as far as Old Ford as I have left my wife and wish to keep as far away as possible.' Old Ford was five minutes' walk from his wife's home. In the earlier days of factory industry wives could not rely upon their husbands to stand by them while they reared their children. A woman had to cling to her mother for moral and material support: 'the extended family was her trade union, organized in the main by women and for women, its solidarity her protection against being alone.' The conditions of the past are reflected in some families today and in the moral code attaching to kinship relations. Thus, 'in Bethnal Green the great triangle of adult life is Mum—wife—husband.' A man is drawn into his wife's family rather than she into his. The close tie between the married woman and her mother is reinforced by the constant exchange of services and can often cause conflict between husband and mother-in-law. This is documented most illuminatingly by Dr. Young and Mr. Willmott, though they are less convincing in their discussion of the husband-wife relationship and neglectful of the rituals of family living. It would have been helpful also had they given some indication of how far the position in Bethnal Green may be representative of kinship relations in other long-settled London boroughs. It may well be an extreme case, for in comparison with cosmopolitan Stepney, Bethnal Green is a curiously closed-in neighbourhood: no main roads run through it to the outer districts, and, as the book shows, many residents can obtain employment not far from their doorsteps.

In the second part of the book the authors follow a number of Bethnal Green families out to Greenleigh, a new housing estate less than twenty miles away in Essex. Cut off from their kinsfolk and living in a district of isolated households, people have to adjust to new circumstances. In Bethnal Green a man has no single status; he meets people in a series of regular face-to-face relationships and is known to them for his personal qualities. He lives in a culture emphasizing solidarity—the value enshrined in relations of kinship. In Greenleigh a man is known not for his personality but for his possessions and it is they that give him his status. He lives in a more competitive society based upon window-to-window relationships. 'Though people stay in their houses, they do in a sense belong to a strong and compelling group. They do not know their judge personally but her influence is continuously felt.' And in fifteen years' time when all the young people are grown up there will be no houses nearby to accommodate them so they must move on to another such estate. If the family were no more than an institution providing services now taken over by the welfare state, this might not matter, but the authors believe that an individual's kinship network links him up with the community. The articulation of households into extended families is as crucial as continuity of residence in explaining the difference between life in Bethnal Green and in Greenleigh.

Thanks to these two books it is now possible for students of kinship in tribal societies to gain new insights into their material from a consideration of analyses carried out in our own society, instead of the flow of ideas being for ever in the reverse direction. For example, the selection of certain kinship relations as legitimizing close ties where it suits personal interest is a feature that has hitherto been glossed over by the study of tribal societies in states of equilibrium. The analysis of English kinship also poses a number of terminological problems. Professor Firth tackles several of these most constructively in his Introduction to the first book. However, it is questionable whether the terms 'affinal kin' and 'consanguinal kin' used in this volume are to be recommended: the first is a contradiction in terms, while the second obscures the fact that the reckoning of kinship is a social phenomenon having nothing to do with biological inheritance.

Both the works under review are particularly well produced and each is a pleasure to read. The L.S.E. monographs are now appearing in a smart new format.

MICHAEL BANTON

**Die jüngere vorrömische Eisenzeit Gotlands.** By Erik Nylén. Stockholm (Almqvist & Wiksell), 1956. Pp 560. Price Kr. 150

35

There are few phases in the prehistory of Gotland still unrepresented in the admirable series of monographs inaugurated over 40 years ago by Almgren and Nerman. In this work Erik Nylén presents the entire late pre-Roman Iron Age (la Tène) material from excavated or closed finds of acceptable authenticity on the island. It is unfortunate, but unavoidable at present, that this material derives exclusively from burials, and that one of the disappointments of the Vallhagar excavations was the failure to discover the settlements relating to the la Tène burials located (mainly) in the Middle Grave Field. Nylén duplicates here his chapter on these graves in the Vallhagar publication, a somewhat wasteful and extravagant procedure, but perhaps justifiable on the grounds of completeness, while the other finds and the circumstances of their discovery are presented anew, all in a single, standardized format. The scholar who uses this book will welcome the change from a medley of obscure papers, difficult of access and of varying excellence, and illustrations often of poor quality and in differing (sometimes even unstated) scales.

This volume is explicitly a material publication, and is quite admirable as the portable museum display it is intended to represent; even the physical handling of the objects could hardly improve on the information conveyed by the excellent descriptions (with full measurements) and photographs and drawings. The only detail that made me unhappy was the use of stippling in the pottery drawings; it fails to reproduce the texture of the fabric or its finish.

Discussion of the material is confined mainly to the ingenious establishment of a convincing four-phase chronological sequence for Gotland itself on the basis of typology, style, find associations and 'horizontal stratigraphy' (is it really impossible to find an acceptable term for this most useful method?), and to a survey of various groups of objects with their foreign parallels. Further interpretation is relegated to a mere 14 pages of script. Though the chronology and type-study are worked out with extreme diligence and any corpus work is a most valuable basis for subsequent research, we are surely justified in demanding more in the way of humanization and historical interpretation in such a work. This period in Gotland abounds in problems, large and small, related both to the history of the island, and, of much greater importance, to the wider picture of Celtic Europe. Gotland, like Britain and for much the same reasons, holds the key to the interpretation of the cultural patterns and their development in neighbouring regions and may help to establish the true nature of the various 'live' connexions concealed by such terms as 'influences' and 'affinities.' These problems cannot be solved by the publication of the bare material, but only by determined attempts at its interpretation; it is therefore disappointing to find nothing here to compare with the interpretative sections of the Vallhagar publication.

Apart from these limitations (mainly sins of omission) and despite its bulk, price and paper covers, this is recommended as a reference work of prime importance to Swedish and European Iron Age studies. Its standards of scholarship and presentation are in every respect what we have come to expect from Scandinavian archaeology, and foreign scholars will warmly welcome its appearance.

STANLEY THOMAS

**Das Ren als Haustier: Eine zoologische Monographie.** By Wolf Herre. Leipzig (Akademische Verlagsgesellschaft Geest und Portig K.-G.), 1955. Pp. 324, 79 figs., 47 tables. Price DM. 20

36

Professor Herre, Director of the Institut für Haustierkunde at Kiel, has written a useful book which includes much of interest to the ethnographer. The author visited the Kautokeino district of Norwegian Lapland in 1942, and between January and April of that year, during the military occupation and under the auspices of the Schulz-Kampfenkel Research Group, he was able to make observations in nine large reindeer herds and among draught reindeer from both the biological and economic points of view. Papers appeared at the time summarizing zoological results (*Umschau*,



1943, Part 12, p. 171; *Zoologische Anzeiger*, Vol. CXXI, 1943, pp. 196-214), also one on the reindeer as a meat animal (*Fleischwirtschaft*, Vol. XXIII, 1943, pp. 89-90). The present work is a comprehensive study of the reindeer as a domestic animal which supplements Arnold Jacobi's *Das Rentier* (1931) in various ways.

Dr. Herre has drawn extensively on the literature of the three major reindeer-breeding areas, and ecology, diseases, herd management, breeding methods and economic uses are all very fully covered as well as biological aspects. Of especial interest to the ethnographer are the chapter on the life of reindeer nomads and the attention paid to archaeological evidence of what has been termed the 'reindeer age' in Europe, particularly to recent North German palaeolithic and mesolithic finds, while the old controversy about the age and origin of reindeer-breeding is summarized and discussed at some length.

The author's main contribution to the discussion of this problem lies in the light he throws on the degree of the reindeer's domestication. Some authors have accepted the description of a 'primitive' stage of domestication as indicating a comparatively recent form of stock-keeping, but this assumption has been based partly on a low degree of submissiveness to man, and in Dr. Herre's view the degree of tameness is only of secondary importance in judging the domesticated status. The latter, he stresses, depends primarily on factors affecting the development of stock over generations, and wild animals may be sporadically tamed without any regular domestication taking place. The similarity between the lives of wild and domestic reindeer, which the author concedes is high, does not, in his opinion, necessarily indicate a short period of domestication, but may be accounted for by a remarkable similarity of environment and by the infusion of wild blood into domestic stock by crossing, which still takes place in some areas. He concludes that evidence pointing to a great age for reindeer-breeding is increasing.

Apart from his own detailed observations, Dr. Herre relies mainly for data on reindeer-breeding practice and herding life in Scandinavia on Johan Turi's *Book of Lapland*, whose representation he considers to have been substantially amplified by M. Utsi in MAN, 1948, 114. Forest reindeer-breeding has not, however, been differentiated as a type existing both in Scandinavia and Siberia, and publications such as Ernst Manker's on Swedish forest reindeer-breeding and those of S. M. Shirokogoroff and E. J. Lindgren on the Reindeer Tungus are ignored.

The author has been disappointed in his search for information on breeding criteria among Norwegian Lapps, whom he has found 'züchterisch höchst uninteressant,' and deduces a lack of knowledge of the inheritance of even simple physical characteristics. In attributing 'primitive' breeding methods to Lappish reindeer-breeding he is no doubt comparing the free unobserved mating inherent in the modern 'extensive' system of herding over wide areas with controlled experiments in selective breeding for specialized production in the Soviet Union, on which he reports fully. But the North Scandinavian 'extensive' system passes the crucial test of economic success, producing, as the author recognizes, a good dual-purpose animal for meat or draught. Closer control of breeding would thus probably not in any case appear practicable or necessary to those engaged in this type of reindeer husbandry.

From Alaska Dr. Herre draws interesting parallels with the early spread of stock-breeding. The dramatic development of Alaskan reindeer-breeding from small introductions of domestic stock at the end of the last century is cited as a modern example of the vast expansion which may proceed from one centre, in this case in surroundings in which wild caribou already existed. Following A. S. Leopold and F. Fraser Darling (*Wildlife in Alaska*, 1953), he regards the unwillingness of the Eskimo to accept herding responsibilities as a major stumbling block to Alaskan reindeer-breeding, and attributes the fact that the Eskimo has never turned the caribou into a domestic animal to the attitude of the Eskimo rather than to any lack of domesticable characteristics in the caribou itself. Little is, however, reliably known of the potentialities of the caribou for domestication.

In spite of some omissions, inconsistencies and errors, this book is an indispensable source of up-to-date information on an animal which is the economic basis of a widespread form of traditional

nomadism and semi-sedentary modern communities. Data from Alaska and Russia are usefully summarized side by side. Unfortunately there is no index, and the reference system fails to identify which work of a particular author is quoted. Misprints occur in foreign names and titles both in bibliography and text, and in Latin plant names (pp. 117-19). Of the 69 photographs the large proportion which have presumably been taken by the author do not appear to have reproduced as well as those appearing in his *Zoologische Anzeiger* article, where detailed descriptions appeared which are lacking here. Besides physical characteristics they illustrate transport and slaughtering methods. The author notes that five publications by H. Pohlhausen, the latest of which was reviewed by Professor Gudmund Hatt in MAN, 1957, 88, reached him too late for inclusion.

R. A. CORKILL

### The Bilateral Network of Social Relations in Kōnkāmā

Lapp District. By Robert Pehrson. *Indiana Univ. Publ., Slavic and East European Series*, Vol. V. Bloomington, 1956. Pp. iv, 128, 12 diagrams, 3 tables. Price \$2.50

37 All of us who had the good fortune to know Bob Pehrson personally got the impression of a young, extremely talented scholar with a deep personal attachment to the people whom he studied combined with a penetrating need for honest, intellectual criticism of his own commitment and of his own observations. The small articles which he published clearly demonstrated the development of one important aspect of his personality—from the first article in MAN, 1950, 256, on 'Culture Contact without Conflict,' a *document humaine* dominated by his passion for the Saames (Lapps) to the present volume which is clearly an important contribution to 'disinterested' theoretical thought.

Pehrson's main theoretical contribution is probably the development of the concept 'bilateralism' on which he focuses his treatment, and which he constitutes as a fruitful anthropological concept because he shows how the bilateralism penetrates almost the whole society, thereby even contributing significantly to the study of the otherwise too neglected field of bilateral (multilinear) kinship systems.

This bilateralism of nearly all aspects of the social system, leading to a horizontal rather than a vertical structure and combined with the ritual kinship relations and the wider aspects of kinship, so excellently treated, clearly makes for a very flexible social system held together chiefly by the sibling solidarity, 'the only cooperate group worthy the name in Lappish social structure.' Here Saamish (Lappish) social systems are obviously related to the general type of the northernmost Indian tribes, outlined by Fred Eggan in his brilliant concluding chapter of *Social Anthropology of North American Indian Tribes*, and a comparative study including the Eskimo and the 'Palaeo-arctic' peoples of North Asia is now needed. Possibly such a comparative study could clear up the genesis of this bilateralism and it would probably contradict Pehrson's hypothesis that the Saamish (Lappish) bilateralism is a correlation to a supposed development from intensive to extensive reindeer-breeding. It might rather turn out to be a function of the earlier fisher-hunter's 'eco-system' and the subarctic biome and habitat, although certainly adapted to the reindeer-breeding. Nor do I feel convinced 'of the possibility that the Lapps earlier had some kind of lineage organization.' Nevertheless Pehrson, and also Ian Whitaker, are obviously right in stressing the importance of the size of the reindeer herd for the constitution and size of the *si'dd*.

Robert Pehrson was a very keen observer and he always makes absolutely clear on what data his deductions are based. His scope is rather modest, and he keeps within his own scope. There can be no doubt whatsoever that his book is the best sociological treatment of a Saame (Lapp) community yet published. And it will be an important impetus to further studies of bilateral societies.

Bob Pehrson tells us about his first meeting with the Karesuando Saames in 1948, when they asked why he wanted to live with them and he replied: 'I want to become a Lapp so that my people may learn something of your people.' He did not feel that he succeeded in 'becoming a Lapp.' But he was closer to it than he imagined himself.

GUTORM GJESSING







(a, b)



(c)



(d, e)



**THE ESKIMO 'ULU' IN THE MALAYAN NEOLITHIC**

*For description see text opposite. Not to scale*

*Photographs by M. W. F. Tweedie*



# THE 'ESKIMO ULU' IN THE MALAYAN NEOLITHIC\*

by

PRINCE JOHN LOEWENSTEIN, PH.D.

*Curator of Anthropology, Raffles Museum, Singapore*

**38** Among the neolithic implements which are of particular interest in connexion with the problem of cultural origins in Malaya a certain type of stone knife or scraper deserves special attention. Though tools of this kind—flat blades of schist or slate, pierced with holes—have been known for some time from several localities in the Malay Peninsula,<sup>1</sup> they have never been compared to examples from outside Malaya. It is the purpose of the present paper to draw attention to such parallels.

Plate D shows five specimens from the Raffles Museum Collection, collected in the Malay States of Kedah, Pahang and Negri Sembilan. These may be described as follows:

(a) Neolithic blade made of a flag of hard coarse-grained schist, from Kampong Siong near Baling, Kedah. No. 35.49. In the shape of a trapezoid. Two holes near the neck, bored from each side. Regular curved cutting edge sharpened by grinding. Maximum length 160 millimetres, width 128 millimetres, thickness 8 millimetres.

(b) Neolithic blade made of a slab of blackish slate, from Kuala Peling, Tembeling, Pahang. Only a cast is extant in the Raffles Museum. No. Z.505. There are two biconical holes, made by hammering; these are placed nearer the cutting edge than the neck. The curved cutting edge seems to have been formed by grinding. Maximum length 150 millimetres, width 155 millimetres, thickness 10 millimetres.

(c) Heavy neolithic blade made of a slab of greenish fine-grained hornstone. Bought from the Malays of Kampong Labu, Tembeling river, Pahang. No. 40.26. Crescent-shaped. Two biconical holes near the neck made by hammering. Rounded neck. Cutting edge rather blunt. Maximum length 114 millimetres, width 343 millimetres, thickness 10 millimetres.

(d) Heavy neolithic blade made of a slab of black fine-grained slate from Jelebu, Negri Sembilan. No. Z.527. Biconical hole, formed by hammering. Curved cutting edge sharpened by grinding. Neck and straight side square-cut. Neck flaked on one side. Maximum length 200 millimetres, width 110 millimetres, thickness 14 millimetres.

(e) Neolithic blade made of a slab of light brown fine-grained schist, from Nyong, Tembeling river, Pahang. No. 36.590. Two large holes formed by hammering from either side. Curved cutting edge sharpened on either side, bearing two small artificial notches (left lower corner). Maximum length 98 millimetres, width 115 millimetres, thickness 17 millimetres.

The most striking features in all these implements, which may be ascribed to a late phase of the Malayan Neolithic, are the circular holes. I. H. N. Evans, who was the first to describe such a tool (the one shown as Plate Dc), suggested that these perforations were presumably for the attachment of a handle, but Tweedie, writing some 25 years later, says that their purpose is problematical.<sup>2</sup> It stands to reason that only a comparison with analogues (if any) can throw light on the nature and origin of these artifacts, which do in fact occur in quantity outside the Malay Peninsula.

They have been found by J. G. Andersson in almost all the prehistoric and early historical sites of Northern China

'everywhere from Honan to the Kuei Te canyon on the border of Tibet,' the most ancient ones dating back to the Yang Shao stage (2200–1700 B.C.). Implements of that kind were most probably made of stone by the country people down into late dynasties and survived in iron among the rural population of China up to the present day.<sup>3</sup> They are not confined to China, but are equally common in prehistoric sites of Eastern Mongolia, Southern Manchuria and Japan.<sup>4</sup> Fig. 1a shows an ancient specimen from the latter country, now in the Museum of Far Eastern Antiquities, Stockholm—a striking parallel to the semilunar knife from the Malay State of Pahang (Plate Dc).

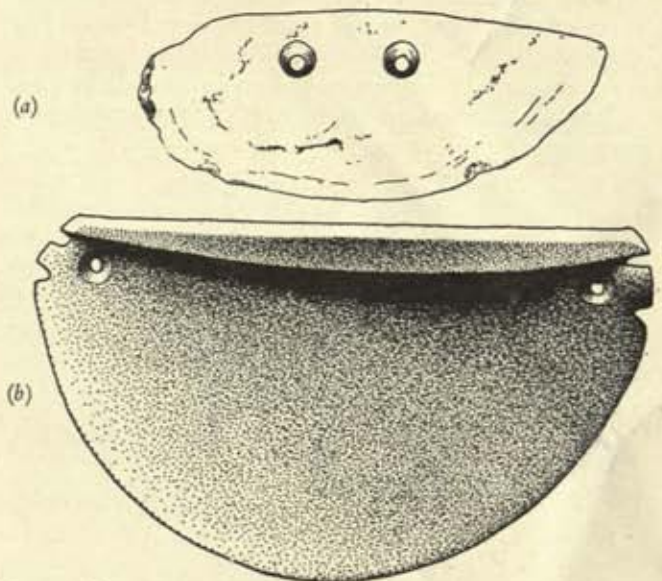


FIG. 1. (a) ANCIENT SEMILUNAR STONE KNIFE FROM JAPAN  
After J. G. Andersson. Not to scale

(b) ANCIENT SEMILUNAR STONE KNIFE FROM THE  
EASTERN WOODLANDS OF NORTH AMERICA

After C. C. Willoughby. Not to scale

All drawings by K. M. Foong

Knowing all this, but no more about the geographical distribution of these implements, an archaeologist may arrive at the conclusion that they made their first appearance in the Yang Shao Culture of Northern China. However, it will become clear later on that Yang Shao was on the receiving end and that the centre of origin of such semilunar and rectangular knives must be sought further north in Siberia, that is to say, in the original homeland of the Mongolian race.

Andersson has in fact shown that this type of knife has a wide range among Mongoloid peoples. It is not only in use among the Asiatic Eskimos, but also among the Chukchi of eastern Siberia, a Palaeasiatic group which was driven

\* With Plate D and four text figures



eastwards by the pressure of more powerful tribes. The modern Chukchi knives are made of iron, but blades of slate and obsidian were found by Bogoras in ancient dwelling sites of this people.<sup>5</sup>

Implements of that kind are not confined to Asia but have also a wide distribution in the New World, which is not surprising, considering the Asiatic origin of Palaeoamerican races. Ancient examples of such tools have been found in both the Americas from Alaska down to Peru. In the latter country they are extant in copper and bronze.<sup>6</sup> In the 'Eastern Woodlands' of North America semilunar knives made of polished stone have come to light, dating from the New England Archaic Period (possibly second half of the first millennium B.C.). An example of such an ancient Indian blade (fig. 1b) has two lateral notches on each side, similar to certain specimens from prehistoric China<sup>7</sup> and Malaya (Plate De). These notches, no doubt, like the holes, served for the attachment of a handle.

Among the Eskimos of North America, the *ulu* or woman's knife is one of the most important tools. The United States National Museum, Washington, possesses a large collection of such knives, gathered from village sites, shell heaps, graves and camps, of which a few examples are shown (fig. 2a-g).<sup>8</sup>

(a) Eskimo *ulu* from Hotham Inlet, Alaska. No. 63765. Leaf-shaped blade of hornstone with primitive handle made of osier, wrapped backward and forward longitudinally and held firmly in place by cross twining and weaving of the same material. The interstices are filled with fish scales. Length 3½ inches.

(b) Eskimo *ulu* from Hotham Inlet, Alaska. No. 63766. Primitive blade of chert or flint, inserted in a handle of wood, showing three grooves at either corner. Cutting edge made by flaking.

(c) *Ulu* of the Eskimos of Point Barrow, Alaska. No. 89675. Large flake of pectolite, a special kind of jade common in this region, used as blade. Length 4½ inches. No handle.

(d) *Ulu* of the Ogulmut Eskimos of Bristol Bay, Alaska. No. 90409. Semicircular blade of greenish slate with two holes. Cutting edge formed by grinding.

(e) Eskimo *ulu* from Point Barrow, Alaska. No. 89688. Blade of drab slate, in a handle of antler. The handle is made up by two separate pieces lashed together and fixed to the blade by a sinew passing through a hole in the blade. Width of blade 3 inches.

(f) Eskimo *ulu* from Kodiak Island, Gulf of Alaska. No. 72540. Large blade of slate inserted in a groove of a cylindrical wooden handle, and held in place by a lashing of braided sinew, passing through two holes in the blade.

(g) *Ulu* of the Eskimos of the mouth of Mackenzie River, Alaska. No. 5813. Iron blade in the shape of a trapezoid, inserted in a handle of walrus ivory. Width of blade 3 inches.

All these knives, or hide-scrapers, as they may be termed more appropriately, are 'modern,' but excavations of ancient Eskimo settlements on islands in the Bering Sea have furnished evidence that the *ulu* belongs to the very earliest tools known to the Eskimos. On St. Lawrence Island, Alaska, Collins found semilunar and rectangular knives of rubbed slate, with or without holes, as well as *ulu* handles made of ivory or wood, some of which were still joined to a blade.<sup>9</sup> These date back to the Punuk Culture (end of the first millennium A.D.) and further still to the earliest stage of the Old Bering Sea Culture (second half of

the first millennium B.C.). As can be observed, the general shape of these knives and the manner of attachment to a handle has hardly changed during 2000 years, though the ancient examples exhibit a high degree of artistic refinement, as far as their carved handles are concerned.

Where is the *point de départ* of this peculiar civilization?

Taking into consideration that vestiges of the Old Bering Sea and Punuk cultures have also been found at Bering Strait and in the Chukchi Peninsula, Collins draws the logical conclusion that the immediate region of origin of the Old Bering Sea Culture must be sought in north-eastern Siberia, somewhere between the mouths of the Anadyr and Kolyma Rivers. Excavations by the Russian archaeologist A. P. Okladnikov around Lake Baikal, on the other hand, have furnished evidence that the ultimate centre of diffusion of this vast cultural complex lies in southern Siberia.<sup>10</sup> As Collins has shown, the full range of implement types encountered in prehistoric Eskimo settlements is already present in the early Lake Baikal Neolithic, that is to say in the second and third oldest stages of this culture—the Isakovski and Serovski. These types include bow and arrow, knives and lances with side blades, needles, needle cases, awls, pottery vessels with conical and rounded bases and finally polished stone adzes, oval stone scrapers and crescent-shaped jade and schist knives.<sup>11</sup> Side-bladed implements of corresponding ancient Siberian shape also occur in Neolithic Yang Shao sites of Northern China, together with crescent-shaped knives.<sup>12</sup>

The early Siberian knives have no holes, but holes are also wanting in many ancient and modern *ulu* blades (fig. 2). Neither holes nor notches are actually essential for keeping a handle in place (fig. 2a, b), but it stands to reason that the handle is altogether a later development and that originally the blade must have been held directly in the hand.<sup>13</sup> The oval stone scrapers and crescent-shaped jade and schist knives of the early Baikal Neolithic may thus be regarded as the true prototypes of the implements under discussion.<sup>14</sup>

However, we cannot ignore the writings of the late Mlle M. Colani, who was a distinguished specialist in the field of Indo-Chinese archaeology. She published three papers dealing with the question of the origin and evolution of the reaping knife, a semilunar iron blade inserted in a wooden handle, used by certain native tribes of Indo-China, the Philippines and Indonesia for cutting rice (fig. 3).<sup>15</sup> Though no better examples of *ulu*-shaped knives could actually be found, Mlle Colani derived these implements from the Hoabinhian 'short axes,' inferring that the Hoabinhians may have known vegetable and rice farming and used the short axes as reaping knives. The difficulty arises when Mlle Colani tries to fit the Yang Shao knives of Northern China into her scheme, suggesting that these were the 'transition forms' ('*Types de passage*') between the Hoabinhian 'short axes' and the modern iron reaping knives.

As has been demonstrated above, the Mongolian knives of Yang Shao type can actually be regarded as 'transition forms,' but it is in Southern Siberia and not in Northern Indo-China that we have to look for their prototypes.



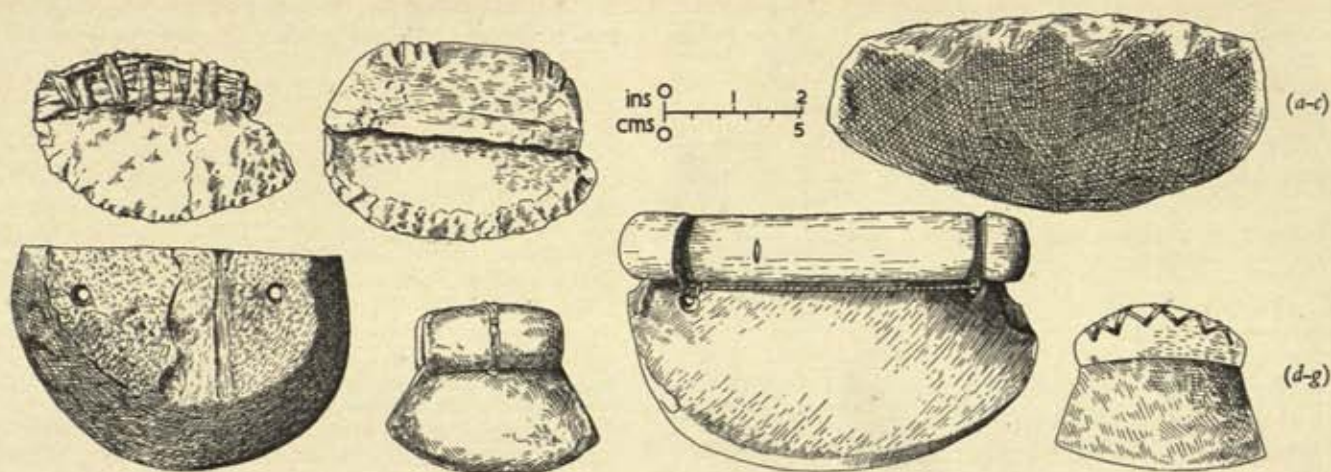


FIG. 2. ESKIMO 'ULUS' FROM ALASKA

*See text. After O. T. Mason*

There seems to be another misconception with regard to these implements, as far as their utilitarian purpose is concerned. The fact that *ulu*-shaped knives are used as rice-cutters in South-East Asia does not imply that this type of tool had served the same purpose in ancient times in other areas, as Mlle Colani tried to prove. It is thus irrelevant in this connexion whether rice was known to the Yang Shao people or to the Hoabinhians and speculations as to the use of semilunar knives in ancient China or Indo-China must remain a mere scientific pastime. In fact it can be shown that this type of knife was used by different peoples for different purposes at different times.

The *ulu* or woman's knife, which the Eskimos of the New World and certain Indian tribes inherited from their Asiatic forebears, is a 'general purpose' instrument, though widely used as a hide-scraper. Another specialized use of this type of knife may throw some light on the utilitarian purpose of the neolithic examples from Malaya.

The Algonkin Indians of eastern America and the Tlingit of the North-West Coast maintain a basswood-fibre industry; they shred the inner bark from certain trees, which they then twist into thread. Most valued by collectors are the ceremonial capes of woven cedar bark made by the Chilkat, a division of the Tlingit.<sup>16</sup> Two implements play an important part in this industry: a bark-scraper (semilunar knife of bone), used in removing the bark from trees, in scraping it down, and in the preliminary process of hackling it; a bark-beater, likewise of bone,

employed to separate the bark into fibres after hackling (fig. 4).<sup>17</sup>

Similar bark-beaters, made of wood, are used by the Malayan Aborigines for the manufacture of barkcloth, which they prepare from the pounded inner bark of certain trees.<sup>18</sup> Cross-hatched stone pounders in the shape of a stump, 6-8 centimetres high, have been found in some quantity at neolithic sites in the Peninsula and Evans has shown that these were most probably bark-beaters.<sup>19</sup> A number of these implements come from the Tembeling area of Pahang, where some of the flat knives have been collected. One knife (Plate De) and a pounder have actually been found at the same site at Nyong.<sup>20</sup> This association of both kinds of tools would suggest that the neolithic people of Pahang used *ulu*-shaped knives as bark-scrapers, as do the Tlingit of North America to the present day.

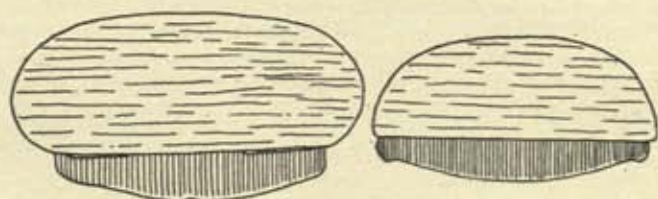
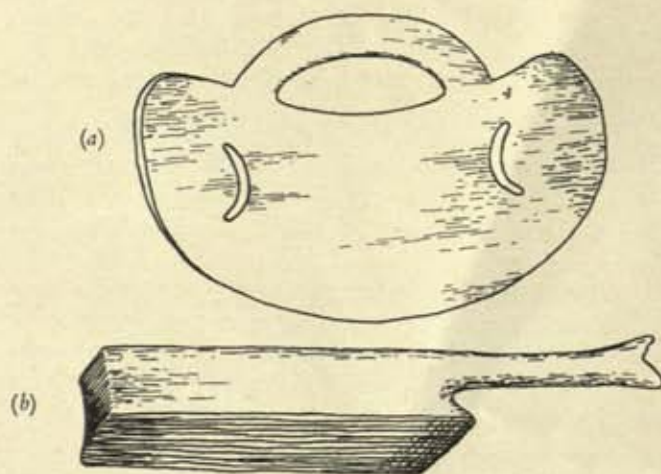


FIG. 3. IRON REAPING KNIVES WITH WOODEN HANDLES FROM INDONESIA

*After van Dapperen. Not to scale*FIG. 4. (a) SEMILUNAR BONE BARK-SCRAPER FROM THE TLINGIT, NORTHWEST COAST  
(b) BONE BARK-BEATER, FROM THE TLINGIT, NORTHWEST COAST*After A. P. Niblack. Not to scale*



## Summary and Conclusions

The Raffles Museum, Singapore, possesses a number of semilunar or quasi-rectangular stone blades, pierced with holes, which may be ascribed to the late neolithic phase. They come from the Malay States of Kedah, Pahang and Negri Sembilan (Plate D).

Stone implements of this type have a wide range in space and time among Mongoloid peoples. They have been found in neolithic sites of Northern China, Eastern Mongolia, Southern Manchuria, Formosa and Japan, as well as in prehistoric Indian and Eskimo settlements of the New World. Examples from St. Lawrence Island, Alaska, dating back to the earliest period of Eskimo culture (second half of first millennium B.C.) show that this type of knife was fixed to a handle, made of wood or ivory, held in place by sinews passing through a hole in the blade or by other means where there are no holes. As 'woman's knife' or *ulu* this tool is still common among the Eskimos of north-eastern Siberia and North America, who use it for various purposes, particularly for scraping hides (fig. 2). Bark-scrapers of similar shape, made of bone, are found among the Tlingit Indians (fig. 4a). In iron this type of knife survived among the rural population of China and South-East Asia; it is used as a reaping knife in Indo-China, the Philippines and parts of Indonesia (fig. 3).

The ultimate centre of origin of these implements must, no doubt, be sought in the Lake Baikal Neolithic of Southern Siberia, which has yielded the full range of implement types encountered in prehistoric Eskimo settlements. These types include flat knives or scrapers of jade and schist, oval or somewhat rectangular in shape, which may be considered as the prototypes of our widely dispersed Mongolian tools. These ancient Siberian knives have no holes and may have been held directly in the hand. The quadrangular adze too, the hall-mark of the Neolithic in South-East Asia, is already present in the early Lake Baikal Culture, attesting that Southern Siberia, rather than South-West China, must have been the original homeland of the forebears of those 'Austronesians' who carried the 'Quadrangular Adze Culture' down into the Archipelago.

Fredrik Barth places the Southern Mongoloid invasions of Further India and Indonesia 'shortly before the year 500 B.C.,' while Heine-Geldern suggested a much earlier date for the migration of the *Uraustronesier* into the Archipelago.<sup>22</sup>

However that may be, the occurrence in the Late Malayan Neolithic of semilunar and rectangular stone knives of Mongolian type and the survival of iron examples of such knives in South-East Asia is additional evidence for those ancient Mongoloid migrations, long surmised by students of physical anthropology.

## Notes

<sup>1</sup> I. H. N. Evans, 'On a Find of Stone Implements Associated with Pottery,' *J. Fed. Malay States Mus.* (Kuala Lumpur), Vol. XII, Part 5, 1928, pp. 133-5, Plate LII, 7; W. Linehan, 'Notes on some Further Archaeological Discoveries in Pahang,' *J. Malay Br., R. Asiat. Soc.* (Singapore), Vol. VIII, Part 2, 1930, pp. 314-17, Plate XII, 2, 3; H. D. Collings, 'Report of an Archaeological Excavation

in Kedah, Malay Peninsula,' *Bull. Raffles Mus., Series B* (Singapore), No. 1, 1936, pp. 5-16, Plate XIX, 1; M. W. F. Tweedie, 'The Stone Age in Malaya,' *J. Malay Br., R. Asiat. Soc.*, Vol. XXVI, Part 2, 1953, fig. 27.

<sup>2</sup> *J. Fed. Malay States Mus.*, loc. cit., p. 133; *J. Malay Br., R. Asiat. Soc.*, loc. cit., p. 39.

<sup>3</sup> They were also made of mussel shell and pottery. J. G. Andersson, 'Researches into the Prehistory of the Chinese,' *Bull. Mus. Far Eastern Antiq.* (Stockholm), No. 15, 1943, p. 223, figs. 121-2, Plates LXX, 1-6, CLXIII-CLXV; 'The Site of Chu Chia Chai,' *ibid.*, No. 17, 1945, Plates XXI-XXIII; 'Prehistoric Sites in Honan,' *ibid.*, No. 19, 1947, Plates LXI, LXII, nos. 1-3, LXXVIII, nos. 1, 3, CXVI, CXVII, CXXIX, nos. 1-6. An Chih-Min, *Acta Archaeologica Sinica* (Peking), Dec. 1955, pp. 27-51, Plates I-VIII. (In Chinese.) Most of the knives figured in this paper are from prehistoric China, but some specimens in stone come from Manchuria (Plate IV, figs. 5-9) and two iron ones from Korea (Plate VII, figs. 1-2).

<sup>4</sup> J. G. Andersson, *Children of the Yellow Earth*, London, 1934, pp. 201-4. For Southern Manchuria see: P'i-Tzu-Wo, 'Prehistoric Sites by the River Pi-liu-ho, South Manchuria,' *Archaeologia Orientalis* (Tokyo and Kyoto), Vol. I, 1929, Plates XIX, XXXV, XXXVIII and fig. 5. Neolithic implements of this type, that is to say semi-lunar knives of slate with one or two holes, have also been found in Formosa: Kiyotari Tsuboi, *Proc. VIII Pacific Sci. Cong. and IV Far-Eastern Prehistory Cong.*, Quezon City, part 1, fasc. 2, section 1, 1956, p. 288, fig. G 58-61.

<sup>5</sup> *Bull. Mus. Far Eastern Antiq.*, No. 15, 1943, p. 226, fig. 86.

<sup>6</sup> *Ibid.*, p. 227.

<sup>7</sup> *Ibid.*, Plates CLXIII, nos. 1-3, 6, CLXIV, nos. 8-10; *ibid.*, No. 19, 1947, Plates LXI, nos. 1-3, 9, 11, 12, LXII, nos. 2, 3, LXXVIII, no. 1, CXVI, nos. 1, 7, 10, 11.

<sup>8</sup> O. T. Mason, 'The Ulu, or Woman's Knife of the Eskimo,' *Ann. Rep. Smiths. Inst.* (Washington), 1891, pp. 411-16.

<sup>9</sup> H. B. Collins, 'Archaeology of St. Lawrence Island, Alaska,' *Smiths. Misc. Coll.* (Washington), Vol. XCVI, No. 1, 1937, pp. 1-431, Plates XII, no. 12, XIV, no. 6, XXII, no. 1, XXXIX, nos. 23-5, LI, nos. 1-7, LXVII, no. 1, LXXXVIII, nos. 9-13, LXXXII, no. 30.

<sup>10</sup> A. P. Okladnikov, 'Archaeological Data on the Ancient History of the Lake Baikal Region' (in Russian), *Rev. Anc. Hist.* (Moscow), Vol. I, No. 2, 1938, pp. 244-60. Summarized by H. B. Collins in 'Eskimo Archaeology and its Bearing on the Problem of Man's Antiquity in America,' *Proc. Amer. Philos. Soc.*, Vol. LXXXVI, No. 2, 1943, pp. 220-35.

<sup>11</sup> H. B. Collins, *ibid.*, pp. 228f., fig. 4, nos. 1, 2.

<sup>12</sup> *Id.*, 'The Origin and Antiquity of the Eskimo,' *Ann. Rep. Smiths. Inst.*, 1950, p. 437.

<sup>13</sup> *Smiths. Misc. Coll.*, loc. cit., p. 350, Plate XXXIX, no. 23; *Bull. Mus. Far Eastern Antiq.*, No. 19, 1947, Plates LXXXVIII, no. 3, CXVI, nos. 3, 5.

<sup>14</sup> Of further interest for the question of ultimate cultural origins in Malaya are the polished stone adzes (with quadrangular cross-section) found in the second and third oldest stages of the Baikal Neolithic (H. B. Collins, *Proc. Amer. Philos. Soc.*, loc. cit., figs. 4, nos. 3, 5; 5, no. 6). Though the chronology of the Lake Baikal Culture is not yet clearly established, it is certain that these implements are considerably older than those from Yang Shao sites in Northern China. Geographically as well as chronologically the Siberian Neolithic stands therefore well at the beginning. R. Heine-Geldern's famous theory on the origin of the 'Quadrangular Adze Culture' in South-East Asia and the southward migration of the *Uraustronesier* still holds good (R. Heine-Geldern, 'Urheimat und früheste Wanderungen der Austronesier,' *Anthropos*, 1932), but A. P. Okladnikov's excavations around Lake Baikal compel us to seek the ultimate *Stammiland* of the bearers of this culture not in South-West China, but further north in Siberia.

<sup>15</sup> M. Colani, *Bull. de l'Ecole Franç. d'Extr. Or.* (Hanoi), 1936, pp. 216-19, 479-81; *id.*, 'Origine et Évolution du Couteau de Moissonneur,' *Proc. III Congr. Prehistorians of the Far East* (Singapore), 1938, pp. 194-9. For Indonesia see J. W. van Dapperen, 'Het Padimesje,' *Nederlandsch-Indië Oud & Nieuw* (The Hague), Jan. 1931, pp. 257-82.



<sup>16</sup> Clark Wissler, *Indians of the United States*, New York, 1954, pp. 58, 195f.; A. P. Niblack, 'The Coast Indians of Southern Alaska and Northern British Columbia,' *Ann. Rep. Smiths. Inst.*, 1888, p. 265.

<sup>17</sup> A. P. Niblack, *loc. cit.*, pp. 311f.

<sup>18</sup> Nelson Annandale and Herbert C. Robinson, *Fasciculi Malayenses*, London, New York and Bombay, 1903, pp. 41f., fig. 13; P. D. R. Williams-Hunt, *An Introduction to the Malayan Aborigines*, Kuala Lumpur, 1952, pp. 42f.

<sup>19</sup> I. H. N. Evans, 'On Slab-Built Graves in Perak,' *J. Fed. Malay States Mus.*, Vol. XII, Part 5 (1928), p. 115, Plates XLII, XLIII. Stone barkcloth-beaters are also known from late neolithic sites in Indo-China, the Philippines and Celebes, in addition to their

widespread occurrence in Polynesia, both in ancient and modern settlements (H. Otley Beyer, *Philippine and East Asian Archaeology, and its Relation to the Origin of the Pacific Islands Population*, Quezon City, 1948, pp. 58-61, figs. 20 a, b, 21).

<sup>20</sup> *J. Malay Br., R. Asiat. Soc.*, Vol. XXVI, Part 2, 1953, pp. 43f.; *J. Fed. Malay States Mus.*, Vol. XII, Part 5, 1928, p. 133; *ibid.*, p. 143, Plate LV, no. 4.

<sup>21</sup> Fredrik Barth, 'The Southern Mongoloid Migration,' *MAN*, 1952, 2.

<sup>22</sup> R. Heine-Geldern, *Anthropos*, 1932, p. 600; *id.*, *Prehistoric Research in the Netherlands Indies: Science and Scientists in the Netherlands Indies*. New York, 1945, pp. 138f.

## BRONZE AGE TECHNOLOGY IN WESTERN ASIA AND NORTHERN EUROPE: PART II\*

by

LEON UNDERWOOD

London

### THE ANNALS OF SENNACHERIB

**39** Though we had better stick to arms-production for the best prospect of keeping at the heels of technological progress, yet it is so rare to have a technical description that we cannot but dally with the Asiatic smiths and examine the description by Sennacherib, the Assyrian, such as it is, before passing on to the bronze moulds of Northern Europe. The King could do no wrong in breaking the rules in his attempt to give more credibility to his performance in the eyes of posterity for whom it was written.

Sennacherib made use of a vast quantity of the precious bronze, acquired by conquest, to gratify his vanity with its splendour on a scale undreamt of by any barbarian conqueror. In the pride of his achievement, though he knew that we would never see his bronze colossi in the 'ruins' of his 'Palace without Rival,' he wanted to tell us of his *tour de main* in casting 46 winged-animal colossi with 1,955 tons of bronze. Actually, Sennacherib's feat in this matter of casting colossal bronzes amounted to an adventurous scaling-up of an existing technique for casting minute weights and he thereby converted a *tour de main* into a *tour de force*. It is a true illustration of the axiom that art reflects life from a leader of a culture depending on force. It put him into something of a dilemma, for there was no theoretical terminology in which to set it down. Its general absence for technical information required teaching to be done by demonstration, affording a measure of security in the arms race of Assyrian times for which we substitute our Official Secrets regulations. There was of course a technical jargon with which demonstrating smiths interpolated their processes and skills, but such jargon carried no accepted meaning beyond each craft centre. In his extremity Sennacherib therefore used an analogy in referring the process to a comparison with the method of casting the half-shekel, a

practice no doubt generally known at the time. He trusted to 'my own wisdom' and 'the advice of my head' that the half-shekel would survive as an Assyrian achievement in setting up a standard of weights and continue to be so produced by posterity.

But I, Sennacherib, first among all princes, wise in all craftsmanship, [made] great pillars of bronze, colossal lions, open at the knees, which no King before my time had fashioned, through the clever understanding which the noble Nim-igi-Kug had given me, [and] in my own wisdom, I pondered deeply the matter of carrying out that task, following the advice of my head [will] and the prompting of my heart, I fashioned a work of bronze and cunningly wrought it. Over great posts and crossbars of wood, 12 fierce lion colossi together with 12 mighty bull colossi, complete in form, 22 cow colossi, clothed with exuberant strength and with abundance and splendour heaped upon them—at the command of the god I built a form of clay and poured bronze into it, as in making half-shekel pieces,<sup>1</sup> and finished their construction (*Annals of Sennacherib*, p. 109, 'The Palace without Rival,' 89 to 19).

A whole series of Assyrian weights in the British Museum range from the larger Talent and Mina weights down to the smallest of three shekels (fig. 1). All were cast by a method which we would call the 'open mould'—a method which could not have been employed for casting colossi.<sup>2</sup> These



FIG. 1. ASSYRIAN THREE-SHEKEL WEIGHT IN THE BRITISH MUSEUM

This is the smallest size available; all larger ones are of the same form, the lion couchant. Length  $1\frac{1}{8}$  inches. Photographs: John Underwood, 1957

\* With four text figures. Part I appeared in the February issue of *MAN* (1958, 13) and Part III, on the European axehead moulds, will appear in the April issue.



weights disclose that inevitable discrepancies of weight in casting the larger ones by the 'open-mould' method had been adjusted after casting, by fitting a heavier or lighter metal handle into sockets cast on the lion's back to receive it. The half-shekel of only 4.2 grammes would be difficult to adjust precisely in this manner after casting. Even if it were done, it would not carry the same assurance of uniformity for users of so small a weight as it might be given by casting it in a closed mould.

Whatever its form—and no surviving specimen is known—the half-shekel seems to have been a small accurate weight, required in numbers for market transactions, to weigh out small quantities of precious material such as gold, electrum and silver and ascertain their exchange value. In this function it was not a coin, for a coin, properly speaking, weighs no other value than its own in gold or silver. Was the half-shekel, therefore, a bronze prototype (forestalling the form of coinage which comes much later)? If then, as Sennacherib's words imply, it was the nearest in its method of casting to that of the animal colossi which he is describing, it must have had some resemblance to the casting of a coin; for the technique employed for casting many coins later would have served very well for animal colossi.

Assyrian winged-animal colossi of granite in the British Museum are of a cubic form, which is likely to have been followed in the bronze colossi. This external five-faces-of-the-cube form lends itself to piece moulding, in pieces comprising, say, the two long sides; front; rear and back—five in all at least. A model for such animal form in bronze, built on the floor of a pit in clay on a supporting foundation of mud brick would need no 'great posts and crossbars of wood' to support it. The wooden supports therefore suggest a framework, or one of several such frame supports, for removing the mould pieces away from the model, which precaution for displacement and replacement would be necessary in the adaptation on such colossal scale of a technique for casting coins or small weights.

The words 'I built a form [mould] of clay and poured bronze into it,' if elaborated, would cover a series of operations on the following lines (cf. fig. 2):

- (1) A model, in clay of from two to three and a half inches in thickness, representing the thickness of the ultimate bronze, built upon a solid core form in mud brick; (2) a mould of five pieces (or more) in clay, taken from the clay model in sections and supported on a framework of 'great posts and crossbars of wood,' enabling them to be removed from the original model; (3) removal of these, to a workable distance, and of the clay thickness (two to three and a half inches) from the model, stripping it down to the core form of mud brick; (4) replacement of the mould pieces in their original position, thus leaving an interval between them and the core form corresponding to the thickness of clay removed, and (5) the bronze to be poured into its place.

Moreover, when we consider the relative weight of the two forms, lion colossi and half-shekels, they lent a certain piquancy to the magnitude of a task which Sennacherib felt justly proud of:

Eight lions, open at the knee, advancing, constructed out of 11,400 talents of shining bronze, the workmanship of the god Nin-a-gal, and exceedingly glorious

—cast in the manner of half-shekels!

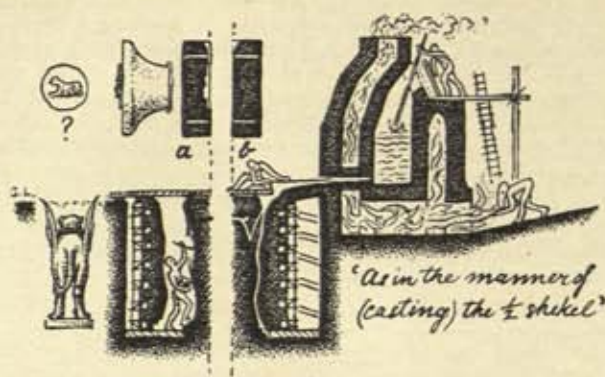


FIG. 2. CASTING OF HALF-SHEKEL AND LION COLOSSUS COMPARED

Left to right, above: conjectural form of half-shekel, die for one (the face) half of its mould, the relief pressed out in clay (a) and the other (plain) half of the mould (b). Below: ground level and finished bronze of winged-lion colossus; the mould for one side of it, supported by 'great posts and crossbars of wood,' withdrawn from the model for a moulder to remove a bronze-thickness of clay from its core, in a pit (a); the mould piece replaced (b) after removal of clay, showing the gap of 2-3 1/2 inches in which to pour the bronze; (above) a foundryman tapping the muffle furnace; on the scaffold, a foundryman with a heat shield and stirring spar, and below a stoker with protective clothing (Sennacherib says that his workmen wore sheepskins and drank oil to enable them to do their work)

What was the size of these bronze colossi? If we take as a guide to the measurements of winged-animal colossi the emplacement of their bases as shown on a scale plan of Sennacherib's palace, we find that they were either 16 or 19 feet long, so—since they each contain 42 1/2 tons of bronze—the thickness in the former case would be 3 1/2 inches, and in the latter case 2 inches.

As has been said, the scale-translation implied in the words '... as in making half-shekel pieces...' is from a mere droplet in a jeweller's crucible to a muffle furnace for melting more than ten million times the volume—a melt the size of a five-and-a-half-foot cube. Certainly the most expedient way of casting duplicated examples of so small a weight with precise uniformity—without necessitating after-adjustment as in the larger weights—may be gathered from the existence of moulds and dies: it involved the two halves of an enclosed flask or bivalve mould such as was used later by coiners. The form of a half-shekel need not have been circular. The matrix for one half of it could have been similar to a positive from one of the negative-relief moulds cut for jewellery (see fig. 3), or it could have been a die

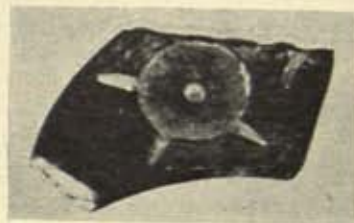


FIG. 3. ASSYRIAN STEATITE MOULD PERHAPS FOR JEWELLERY  
The normal way of making small reliefs was to sink them negatively in an easily cut stone such as steatite; from these waxes could be taken for casting in metal. About half actual size





FIG. 4. ASSYRIAN ASS'S HEAD DIE IN BRONZE

This die is for a relief on a flat field which could well have served in the manner described in fig. 2. The stem behind is buried over from being struck with a mallet. Length  $1\frac{1}{4}$  inches.

(in positive form) similar to the ass's head on a flat field (fig. 4).

Impressions from relief sunk in steatite would present a relief form on a flat field of wax which could be cast in bronze examples. The master of weights and measures would then have a number of bronze dies similar to the ass's head die in the form of positive reliefs on a flat field. The depth of impression made by these dies pressed into stiff clay would be uniformly controlled by the flat field. All then required for the form of the other half-mould would be a box-like depression in clay deep enough to contain the remainder of 4.2 grammes of bronze. The back half of a flask mould would have a channel groove for the jet of metal which would be removed after casting.

Sennacherib's terse words of description must have meant something like this on the colossal scale. His achievement was, in its magnitude, a real *tour de force*—the making of the two walls of an enormous compartment, one of which might be removed and replaced when a bronze-thickness of clay had been removed from the core form. The mould pieces, replaced, would leave a compartment in which to pour  $42\frac{1}{2}$  tons of bronze. But then his words, '... and finished their construction,' imply that the lion was cast in pieces, as seems reasonable. Again, in this case, the cast pieces might be supported when assembled on 'great posts and crossbars of wood.'

What is entirely improbable is that a crucible of any great size in the whole or part could have been made at this date to be mobile. The method employed—whether the  $42\frac{1}{2}$  tons of bronze were poured in one or divided for five or more section castings—must have been that of a static crucible or furnace, most probably a furnace built on ground level near to one side of the pit in which the mould stood beneath. The furnace could then be tapped for the metal to run out through conduits into the mould below.

Though Sennacherib was more unable than unwilling to say more clearly the way in which this was done, it was surely a technical performance which might test the skill of sculpture foundry-masters today.

## Notes

<sup>1</sup> The word for 'pieces' is not in the cuneiform text.

<sup>2</sup> Unfortunately, scaffolding for museum redecoration frustrated my intention to make a complete examination of all the available Assyrian weights in the Department of Western Asiatic Antiquities and look for any variations.

## SHORTER NOTES

#### An Iron Mining Tool from Uganda, with a Note on Rhodesian Parallels. By E. C. Lanning, Masaka, Uganda. With two text figures

40

Following enquiries made at Butiti in Toro District, an old smith described a tool, not unlike a pick, which he called *emissuma*. This is said to have been used by miners in Toro in the digging of shafts sunk for the purpose of extracting iron ore. No such implement having been recorded within the Protectorate, the informant was asked to prepare a full-size model (fig. 1b).

Two years later, in 1956, following investigations at Kasasa near Lukaya in Masaka District of shafts similar to those of Butiti, an implement was found on the surface within the area covered by the shafts (fig. 1a). As a result of bush-clearing the ground had been disturbed here and there to a depth of some 10 inches, hence the exact provenance of this tool is uncertain. The implement puzzled local natives. On examination, though shorter in length, it was found to be in other respects almost identical to the Butiti model *emissuma*.

Both implements are cigar-shaped and are best described as small pick-axes. Dimensions in millimetres are:

	Cross section at widest part	Overall length
No 1. Butiti (Uganda Mus. E 53.268)	19 × 15	247
No. 2. Kasasa (Uganda Mus. E 57.07)	23 × 19	208

According to the maker of the Butiti model, the pick was

tanged or driven through a short wooden haft some 42 centimetres long, apparently suitable for wielding in a confined space. This is an obvious advantage over the traditional digging spear of Buganda and Toro.

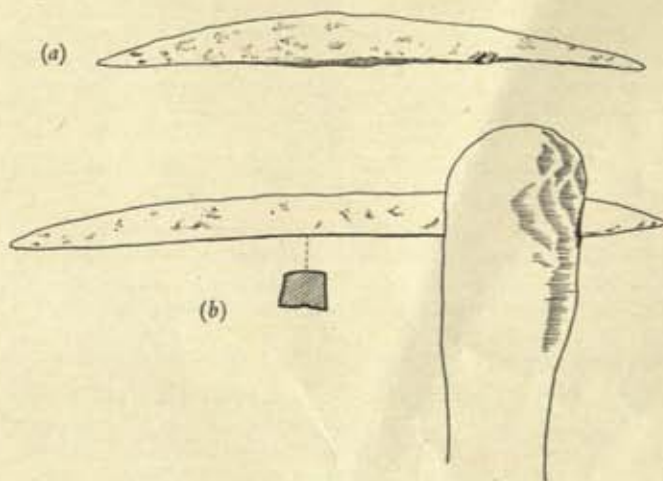


FIG. 1. (a) PICK-HEAD FROM KASASA  
(b) MODEL 'EMISSUMA' FROM BUTITI  
Scale  $\frac{1}{2}$



On first examination the pick suggests a possible adaptation of an indigenous technique to an imported pick-axe. However, if this were the case it could be expected that such an implement would be better known to blacksmiths of today. This does not appear to be so. While the one informant claims that this tool is of a type used for the sinking of shafts for iron-ore-extraction in Toro, the Kasasa pick comes from a group of shafts where there is neither evidence of iron workings nor of tradition concerning the shafts themselves. However this may be, the fact remains that both picks are associated with this particular type of shaft, the Butiti group being some 110 miles, in a straight line, from that of Kasasa.

Groups of vertical shafts are now known to occur at various sites in Uganda<sup>1</sup>; those in Toro District at Butiti as well as at Lugombe and Mugangula; in Buganda Kingdom in West Mengo at Ntanda in Ssinga County; in Masaka District near to Kako and at Kasasa, both in Buddu County; and in Mubende District at two places in Buyaga County.<sup>2</sup>

Each of these groups appears to be composed of at least 50 or more shafts. Invariably the shafts have been sunk close to one another, from five to ten yards apart and, in most cases, without following any alignment. It is only at two groups, at Ntanda and Kasasa, that it has been noted that a few shafts have been symmetrically arranged. Despite the distances separating the groups one from another the dimensions of all shafts are similar. Each one is circular, having a diameter which is usually about two-and-a-half to three feet, though a variation in a few cases of up to four feet has been noted. Depths vary from 10 feet, where erosion and caving-in is evident, to a more general depth of 25 to 30 feet. It has been reported that a number of the shafts at Kako tend to bell out at the bottom, this feature being most prominent on the northern face of the shaft, the uphill segment.<sup>3</sup> In two instances there appear to be underground connexions. At some groups footholds are cut down the sides.

The original purpose of these shafts is unknown to present inhabitants. At Butiti, Buyaga and Ntanda excavations have penetrated lateritic ironstone. At Butiti only is there some definite oral evidence that they were used, at least in recent times, for the mining of iron ore. Nevertheless it is still open to question whether these shafts were in fact originally designed for such a purpose.

At some places certain of the shafts reveal substantial chalk deposits. Such *enoni* was used for smearing on hut walls in pre-European times. It is possible therefore that this extraction was one of the reasons for mining activity at some of the sites. In neighbouring areas there is ample evidence of rougher, wider and shallower pits from which both iron ore and chalk have been won by modern smiths and, as far as memory can recall, their forebears. These workings are quite dissimilar to the above-mentioned circular and vertical shafts.

There has been no thorough investigation made of any of the groups nor of any number of shafts within a group.

Since writing the above note my attention has been drawn to a report by Mr. Bernard Fagg of similar tools found in pre-European mining shafts in Northern Nigeria. They are displayed, with two original wooden hafts, in the Jos Museum (fig. 2).

Following examination of the Kasasa pick, Mr. R. F. H. Summers, Curator of the National Museum of Southern Rhodesia at Salisbury, has remarked on the apparent close relation to the gads of Southern Rhodesia. His observations are appended:

This *emissuma*, found near a mine shaft at Kasasa in Uganda, resembles the iron gads frequently found in pre-European mine workings in Southern Rhodesia.

Twelve Rhodesian specimens, now in the National Museum of Southern Rhodesia, have been examined but all show signs of wear on one or both ends whereas the Uganda specimen is

quite fresh. Moreover, most Rhodesian gads taper equally on all faces so that they have a cigar-shaped section. One specimen had one face flat and the others curved and so is very similar indeed to the one from Uganda; in these cases the dimensions in millimetres are almost identical:

	Cross section at widest part	Widest part to end	Overall length
Uganda (Kasasa)	23 × 19	120 } 88 }	208
Southern Rhodesia, Ka-Ka Mine, Que, Que	24 × 18	90	161

Both ends of the Southern Rhodesian specimen are battered, one very severely.

The wear on the Rhodesian gads, the wear on stone hammers and finds of wooden handles from the Rhodesian 'ancient workings,' all suggest that in the South gads were used as wedges held in handles and hammered, rather than swung as are sampling picks today.

However, the shape of the Uganda specimen suggests that it was used on something softer than quartz, the usual gold-bearing rock in Rhodesia, and was possibly swung like a modern pick.

The Rhodesian specimen could be anything from 1000 to 100 years old, but unfortunately there is not enough evidence to show if any development took place during that long period. Typologically, the Uganda type of pick, as evidenced by this surface find, is ancestral to the Rhodesian types, but this gives us no clue as to the age of the specimen.

It is interesting that mining technique in Uganda consists in the sinking of shafts; the same technique is found in parts of Southern Rhodesia, but there open workings (like very deep and narrow trenches) are more common than shafts.<sup>4</sup>



FIG. 2. MINING TOOLS IN JOS MUSEUM

#### References

- <sup>1</sup> E. J. Wayland, 'Some facts and theories relating to the geology of Uganda,' *Govt. Press, Uganda*, 1920; E. C. Lanning, 'Notes on certain shafts in Buganda and Toro,' *Uganda J.*, 1954, pp. 187-90.
- <sup>2</sup> E. C. Lanning, 'Shafts in Buganda and Toro,' *Uganda J.*, 1956, pp. 216-17.



<sup>3</sup> C. A. E. Harwich, 'The Kako Shaft.' Unpublished paper. Geol. Survey Dept., Uganda. 1946.

<sup>4</sup> F. P. Mennell and R. F. H. Summers, 'The ancient workings of Southern Rhodesia,' *Occ. Pap. Nat. Mus. S. Rhodesia*, No. 20, 1955.

### 'Comparative Studies in Society and History': An International Quarterly

**41** In October, 1958, will appear the first issue of a new international quarterly, *Comparative Studies in Society and History*, to be published by Mouton of The Hague with several American universities as sponsors. The purpose of the journal is to serve as a clearing house for substantive work on problems that are common to any two or more of the numerous branches of study dealing with man's life in society. It will treat any subject that bears on the explanation of stability or change within a social structure, including ways of thought or expression, and it will deal with forms of cultural interchange. It is not the intention of the Editors merely to popularize work that is already well known to specialists. They invite contributions from fresh research in which the problems at issue are clearly formulated. They hope to achieve some testing of the ideas of the theoretical social sciences in regard to longer intervals of time and larger social units than is customary. Comparison, if not drawn within an article, can be attained by setting together articles on comparable material.

A board of Consulting Editors has been drawn from a number of universities representative of comparative politics, comparative literature, economics, sociology, anthropology, and various fields of history. Anthropology is represented by Raymond Firth (London), M. J. Herskovits (Northwestern), A. L. Kroeber, and E. Wolf (Virginia). The Editorial Committee consists of Sylvia L. Thrupp as Editor, G. E. von Grunebaum, Everett C. Hughes, Edward A. Kracke, Jr., Max Rheinstein, Edward Shils and Sol

Tax. Correspondence regarding contributions should be directed to the Editor, Box 222, Faculty Exchange, University of Chicago, Chicago 37, Illinois.

**'Anthropologischer Anzeiger.'** A note by Dr. Hermann Lehmann, Department of Pathology, St. Bartholomew's Hospital, London

**42**

The *Anthropologischer Anzeiger* was founded by Professor Rudolf Martin in 1924. Its purpose was in the first place to be a recorder of the events of international anthropology. There was an exhaustive bibliography, complemented by selected critical summaries of important papers and books, reports on conferences, announcements of coming events and general news concerning developments in research and teaching. In addition there was a section with small original contributions. When Martin died after barely two years his successor in the Chair of Anthropology in the University of Munich, Theodor Mollison, also continued his work as an editor of *Anthropologischer Anzeiger* until 1943 when the difficulties of wartime brought the journal to an end with fascicles 1/2 of the nineteenth volume.

Professor W. Gieseler of Tübingen and Professor E. Breiter of Frankfurt am Main have now followed on where Professor Mollison left off, and the first number of the new series contains a moving obituary of Mollison, who died in 1952. *Anthropologischer Anzeiger* is intended to appear in four numbers per annual volume, of about 80 pages each. The bibliography has been divided into new sections to do justice to recent developments in physical anthropology. There are special divisions for serology, diagnosis of paternity, and human genetics; pathology, eugenics, etc., are fully considered. The editors hope to include in due course references to work which has been appearing while the *Anzeiger* was lying dormant. One wishes the *Anthropologischer Anzeiger* all success, and may its resurgence be symbolic for the healing of wounds that German anthropology has received in the fateful years of Hitler.

## CORRESPONDENCE

**The Ethnography of the Lapps.** Cf. *J. R. Anthropol. Inst.*, Vol. LXXXVI, Part 1, pp. 87-94

**43** SIR,—The ethnographical remarks that preface the recent paper by Allison, Broman, Mourant and Ryttinger on 'The Blood Groups of the Swedish Lapps' in the *Institute's Journal* stand in need of correction.

1. *Divisions of language and economy.* Central and Southern Lappish 'languages' have been incorrectly equated by the authors with 'mainly . . . nomadic reindeer-breeder . . . or true Reindeer Lapps,' and Northern Lappish with 'the other, rather more miscellaneous [group, including] the Sea Lapps, Fisher Lapps, Skolt Lapps, and Kola Lapps.'

The authors apparently base their remarks on Lundman (1946). Note should be taken of what Lundman himself wrote: 'Roughly speaking, it is thus possible to distinguish a high-skulled southern group and a low-skulled northern group, so that in the following I shall in this sense often speak of southern Lapps and northern Lapps. As the most pronounced northern Lapp populations are evidently fishing Lapps, and the southern Lapp populations are descended almost exclusively from nomad Lapps, I shall sometimes, accordingly, speak of the mountain or reindeer-breeding type of Lapp and the coastal or fishing type of Lapp. (All this is of course inadequate, but can scarcely, after these explanatory remarks, give rise to misunderstanding.)' Even as stated by Lundman himself, and even with the qualifications imparted in the words which I have italicized, the ethnographical implication remains extremely debatable; by misrepresenting Lundman in their summary, the authors have achieved a 'misunderstanding.'

2. *Distribution of Lappish populations.* The confusion above noted is,

of course, carried into the following figures: 'as regards the . . . true Reindeer Lapps, rather fewer than 9,000 live in Sweden, about 18,000 in Norway and 1,500 in Finland.' Any twofold division of language (more correctly, of dialect) is insufficient in any case. The appellations 'Northern,' 'Central' and 'Southern' are all right, but they have until recently been used of the Swedish Lapps alone. To follow Professor Collinder, when we wish to include all Lappish groups, there are the so-called Eastern, Northern and Southern Lappish languages (dialects).<sup>2</sup> This is a useful simplification of Konrad Nielsen's earlier divisions,<sup>3</sup> not least because the appellations do, in fact, relate to eastern, northern and southern geographical areas. Eastern Lappish comprises the sub-dialects of Inari (of the Finnish Fisher Lapps), Skolt and Kola (both Russian groups). Northern Lappish, 'taking the term in a wide sense' (Collinder), is spoken throughout Finnmark and Troms provinces in Norway, by the Reindeer Lapps of Finland, and in the Swedish province of Norrbotten, with the exception of its southernmost part (Arvidsjaur). Southern Lappish is restricted to those Lapps (numerically not formidable) living in Norway and Sweden approximately south of a line joining Saltfjellet and Arvidsjaur. The figures given by Allison *et al.* for 'the true Reindeer Lapps' fall only a few thousand short of the probable Lappish 'world population.' The essential proportions of Lappish population, economy and language are these: the majority of the world's Lapps are not engaged in reindeer-breeding and belong to the Northern Lappish language group—including all the Coast Lapps (Norway) whose total alone must account for nearly half of the Lappish world total. The Lapps whose primary livelihood is reindeer-breeding are found in all of the four countries (Russia, Finland, Norway and Sweden) and three Lappish



'language' areas. In Norway they form a small minority of the Lappish population (hardly more than 1 in 10), most of them speaking Northern Lappish. In Sweden they are 33 per cent. of the Lappish population,<sup>4</sup> most of them speaking Northern Lappish.<sup>5</sup> In Finland they number c. 1,500 of a national Lappish total of c. 2,500; all the Finnish Reindeer Lapps speak Northern Lappish.<sup>6</sup> I am unable to cite figures concerning the Russian (Kola and Skolt) Lapps.

3. *Regional separateness.* The authors write: 'Apart from recent population movements, the Swedish Lapps—especially those dwelling in the southern Lappmarks—have been to a large extent separated from the Norwegian Lapps of all groups by geographical barriers. This separation for a long period of time is undoubtedly the cause of certain linguistic and cultural differences between the Lapps of the two countries, which have been remarked by all observers (e.g. Collinder, 1949 [*The Lapps*]).' The reference to 'all observers' will astonish anyone seriously acquainted with the field of study; no page reference is given to Collinder (1949), and could such a reference be given, that would be a matter to astonish us no less. Granted the irrelevance to their purposes of recent population movements, the authors still quite misplace their emphasis. Certainly, geographical dispersion and isolation, differences in economy and dialect, incline one ordinarily to emphasize the parochialisms of the individual Lappish groups. Accordingly, within either Sweden or Norway certain local Lappish groups may ordinarily have never met individuals from other groups in the same country. But the point that requires making here is that lateral (west-east) arteries of communication have run, for example, between Karasjok (Norway) and Utsjoki (Finland), Kautokeino (Norway), Karesuando (Sweden) and Enontekiö (Finland), and between Lofoten, Ofoten, Tysfjord, Røros (Norway) and adjacent Swedish Lapp districts. This situation is one with a long history, from which have arisen not only linguistic and 'cultural' similarities, but also blood and guest relationships.

Where inter-area contact and inter-group affinities have been strongest, it has often been across national boundaries. Where these boundaries have followed an approximately parallel course to the coast (as between Norway and Finland, Norway and Sweden) they have crossed either annual migration routes or two complementary ecological zones or both. Earlier, the Lapps could, and did, ignore the national limits; since the middle of the nineteenth century this has not, in all areas and circumstances, been possible.<sup>7</sup>

Tromsø Museum, Norway

ROBERT PAINE

#### Notes

<sup>1</sup> B. Lundman, 'On the Origin of the Lapps,' *Ethnos*, 1946, Parts 1 and 2, p. 74.

<sup>2</sup> Björn Collinder, *Fenno-Ugric Vocabulary*, 1955, p. x.

<sup>3</sup> Konrad Nielsen, *Lærebok i Lappisk*, Vol. I, 1926, pp. vii-ix.

<sup>4</sup> Karl Nickul, *Report on Lapp Affairs*, Finnish State Commission, 1952, p. 16.

<sup>5</sup> Ernst Manker, *De svenska fjälllapparna*, 1947, pp. 64-67.

<sup>6</sup> Karl Nickul, *op. cit.*, pp. 23, 60.

<sup>7</sup> See J. G. Elbo, 'Lapp Reindeer Movements across the Frontiers of Northern Scandinavia,' *Polar Record*, Vol. VI, No. 43, 1952, *passim*.

#### Attrition of the Teeth among Tibetans. Cf. MAN, 1957, 227

44

SIR,—H.R.H. Prince Peter of Greece and Denmark has made a fascinating contribution to dental anthropology in his 'Attrition of the Teeth Among Tibetans,' the facts of which will doubtless stimulate further investigation by more academic minds than mine.

I was, however, much puzzled by one of his observations in

particular. He remarks that, among 'other abnormalities in Tibetan dentition' he found 'extra teeth growing behind the others (in some cases a whole second row in the upper maxillary).' This is not only remarkable, but unaccountable in the light of my knowledge. Supernumerary teeth, one or two in certain areas of the jaws—are a common aberration, explained by division of the tooth germ during tooth formation, or by the appearance of an extra tooth bud. Seldom do more than one or two supernumerary teeth appear and these are usually malformed, or less well formed than those of the proper dentition. When, too, the dental arches are crowded, teeth may be placed inside others—having failed to find room in normal alignment—giving the appearance of a second row of teeth, for which they may be mistaken, by others than dentists; but here there is only the proper complement badly disarranged. At no time, however, has 'a whole row' of supplementary or supernumerary teeth been recorded.

There have been stories of third dentitions occurring from time to time, none of them substantiated, though these have always been said to succeed the second dentitions and not to be contemporary with them.

Apart from such claims, never—I repeat—verified, no records exist (to my knowledge) of such conditions as Prince Peter describes. It would be of considerable interest to me, and importance to those engaged in serious work of this nature if he could enlarge upon his observations.

The conditions mentioned, excessive attrition and bruxism are readily explained by the diets and habits of the Tibetans as detailed in this article. Nothing, however, suggests a reason for so remarkable a phenomenon as an extra dentition. Nor can I imagine any biological, physiological or ecological influences which are likely to produce it, either in Tibet or elsewhere.

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EDWARD SAMSON

45

SIR,—H.R.H. Prince Peter of Greece and Denmark in his article 'Attrition of the Teeth among Tibetans,' when quoting Professor P. O. Pedersen, implies that the attrition is entirely due to environmental factors. It is highly probable that such is not the case.

The teeth of all mammals are covered with a complex series of cusps and ridges. These are not distributed haphazardly, but are so arranged as to shear upper against lower, in normal chewing action. For this to be possible there is a complex inter-relationship between shape and position of the teeth, the morphology of the temporomandibular joint, and the patterning of muscle activity. Provided that this exists, wear or attrition takes place and provides a self-sharpening mechanism for the cusps of the teeth. This occurs in almost all races of man as well as all other orders of mammals.

It is only in a highly civilized society, such as that of Western Europe, with extensive inter-marriage between different racial groups, that this relation between joint, muscles and dentition is lost. The lack of attrition in such cases is not due primarily to lack of use of the teeth, but to the inability, due to malocclusion, to use the teeth as evolution originally determined. In Europeans possessing a good dentition, with freedom for lateral movement of the jaw, attrition occurs as in other racial groups.

We suggest, therefore, that attrition is a normal feature of the teeth of all mammals including man. Its low incidence and variability in European races is an anomaly due to malocclusion of the teeth. The degree of such attrition is affected to only a minor extent by masticatory habits.

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C. F. BALLARD  
J. R. E. MILLS



## REVIEWS

## GENERAL

**Oriental Religions in Roman Paganism.** By Franz Cumont, authorized translation. New York (Dover Publ.), 1956. Pp. xxv, 298. Price \$1.75. **The Mysteries of Mithra.** By Franz Cumont, transl. from the second revised French edition by Thomas McCormack. New York (Dover Publ.), 1956. Pp. xiv, 239. Price \$1.85.

46

It is now 61 years since Cumont published the first volume of his *Textes et monuments figurés relatifs aux mystères de Mithra*, a book of such research, scholarship and, what is still more valuable, perception, that it has become, for all students of the history of religions, a classic. The more popular *Mystères de Mithra* of which the second of the two volumes issued by the Dover Publications is the translation, followed in 1902, and was succeeded by the comprehensive study entitled *Les Religions orientales dans le paganisme romain* in 1906. Within the last half-century interest in the centuries immediately preceding and following the Christian first century has been further quickened and stimulated by discoveries that must have delighted the veteran Belgian scholar. The study of syncretism and that form of it which we know as Gnosticism has been enriched by translations of early Coptic Gnostic and Christian documents, Manichaean texts, the Nag Hammadi manuscripts, Mandaean texts and the astonishing finds in the Jordan Valley and Cairo Geniza. It has become fashionable, especially in some theological centres, to belittle such methodical research as that which Cumont, in his ripe scholarship, employed. The prophets of psycho-analysis have traced much mythology to primitive and universal symbolic tendencies in man's unconscious mind. The identification of such symbols with mystical experience is a process which has endeared itself to many who battle against mounting resistance to dogmatic churches and creeds, and by such people Jung is regarded as a saviour as well as a scientist. To such, Cumont's writings come as a salutary reminder that, as he wrote: 'even if we oppose tradition, we cannot break with the past that has formed us . . . As the religious history of the empire is studied more closely, the triumph of the church will, in our opinion, appear more and more as the culmination of a long evolution of beliefs.' (*Oriental Religions*, p. xxiv.)

Interest in Mithraism in this country was heightened not long ago by the discovery of a Mithraeum in the heart of London which attracted crowds of sight-seers. The present cheap and accessible edition of the two volumes under review should, therefore, have a wide circulation and Dover Publications are to be congratulated upon their energy in bringing out such works as these, and Maimonides's *Guide to the Perplexed* which I note is also upon their list. Mr. Grant Showerman's introductory essay to *Oriental Religions* upon 'The Significance of Franz Cumont's Work' is, by the way, admirable and adequate.

E. S. DROWER

**Music in Primitive Culture.** By Bruno Nettl. Cambridge, Mass. (Harvard U.P.) (London agents: O.U.P.), 1956. Pp. xviii, 182. Price £2.

47

This is a very welcome book indeed. In a field where too many of the workers seem to take a pride in having stables full of hobby-horses, the author manages to give a dispassionate account and appraisal of the different existing theories and to put his own sensible views without ostentation. With its 60 musical examples and a very extensive annotated bibliography in addition to the notes given with each separate chapter it is a useful guide to newcomers to this newly established discipline. Coming from America the book naturally gives more data from that continent than from other parts of the globe (excepting Africa), also because, generally speaking, the material collected among the American Indian tribes and the Eskimos is more systematically arranged and more easily available than that from elsewhere, although Africa now seems to become a very good second.

It is interesting and valuable to have the two main schools of ethnomusicology, the German and the American, explained side by side. Incidentally it shows some of the traps into which unwary

students might easily fall. How different is the interpretation of two seemingly identical terms, the German 'Kulturkreis' and the American 'culture area,' the former largely a theoretical concept, the latter based on factual anthropological observation. Under each heading scholars have made important though widely divergent contributions, but, on the whole, the latter tends to be more level-headed. The author gives both their full due, having by descent and by upbringing, so to say, a foot in each camp.

His own working hypothesis as to the origin of music, stated on p. 136, is well worth noting. In very remote times there must have been what he calls 'an undifferentiated method of communication which was neither speech nor music, but which possessed the three features they hold in common, pitch, stress and duration. . . . From this common basis both music and speech developed in course of time. This theory does not entirely invalidate Darwin's 'mating cries,' nor Buecher's working-song hypothesis, nor Nadel's thesis that music was originally a special means of communicating with the supernatural. It is worth mentioning that in the mediæval Sanskrit theory of music speech and music are described as two manifestations of the same primordial sound, *Nāda*, which, in the form of words, rules our daily life and, in the form of music, leads us to the realm of the gods.

To remain in India for a moment longer: the Indian theoreticians seem to have anticipated Sachs and von Hornbostel by roughly 2000 years, as the *Bharatanāṭyaśāstra*, dating from about the beginning of our era, already classifies musical instruments as *tata*, *ghana*, *avanaddha* and *susira*, which corresponds exactly to their four categories (p. 92)—chordophones, idiophones, membranophones and aerophones.

The author's interesting chapter on Afro-American musical relationship could perhaps be taken one step further, if Father Jones's thesis that American Jazz now influences African styles of rhythm is as correct as it seems to be.

It would have been illuminating if, while describing the notched-stick idiophone (pp. 96, 97), especially the Mexican examples shaped like animals, the author could have drawn attention—if only in passing—to the presence of raspers in the shape of tigers in Chinese temple orchestras.

The way in which the author demonstrates the dangers of a too wide application of symbolism is timely and necessary. On p. 103 he says: 'The specialized roles of certain instruments in ritual and ceremony are widely recognized, although it is doubtful that the same instrument carries an identical symbolic meaning in different cultures.' Many of the workers in this field would do well to heed this warning.

Both ethnomusicologists and anthropologists will, of course, find much with which they have long been familiar, but neither should miss reading this book, because, even for old hands, it contains much useful, new and stimulating material.

A. A. BAKE

**Notes on Prehistoric and Early Iron in the Old World.** By H. H. Coghlan. *Occ. Pap. on Technology*, 8. Oxford (Pitt Rivers Museum), 1956. Pp. 220. Price £1 5s.

48

Mr. Coghlan needs no introduction to readers of MAN. As a practising engineer and an active museum curator he has a stereoscopic view of ancient technology. He has made full use of this to give us, over a number of years, several valuable contributions particularly to the study of metals, and among them this last is probably the most important and certainly the most impressive.

Mr. Penniman, joint editor of the series, allowed many specimens from the Pitt Rivers Museum to be analysed by one of the industrial concerns most qualified to do so in this country, and he has in Mr. Allen (who contributes an interesting note on an Eskimo knife) a resident technological watchdog of much promise. Mr. Coghlan himself has much cause, in general, to be pleased with the quantity of response in recent years to his continued pleading (e.g., in his paper



to Section H of the British Association in 1950; see MAN, 1951, 156) for more attention from archaeologists to the technology of their metal finds. Qualitatively, there is still much room for improvement, it is true, and here Mr. Coghlan most properly shows us how things should be done.

In his consideration of the ores, fuels, tools, furnaces and techniques available to the early miners and extractors of iron, Mr. Coghlan is necessarily dependent to a large degree on information much scattered in time and space, and uneven in quality. He picks his way through it carefully. His own comments are always reliable even if they sometimes cannot be very helpful. Similarly, he is right to devote a fair-sized section to meteoric iron if only to establish, once and for all, that it could never have been of any general technological importance.

It is obvious from this book—as it was from Mr. Coghlan's *Occasional Paper No. 4* on copper technology—that if our knowledge of these things is to advance reliably we must actually *do* them again and see what happens. Not only is it essential for the excavator to have specialist metallurgical advice in uncovering a furnace; it must be rebuilt—or a copy made—and a properly controlled reduction must be carried out in it. Only then can we talk with any real justification about the temperatures and products of which it was capable. Even so we should never forget, as Professor Childe reminds us, that this capacity may not have been fully realized.

When we come to the finished products themselves the situation is somewhat better. The craft of working wrought iron functionally is still with us, in the Old World. For various reasons the direct translation of primitive techniques, from the modern New World to the ancient Old, may often be inadmissible for purposes of comparative study. But our blacksmith and ironworker today are descended in a direct line from the tradition with which we are concerned in this book.

Nevertheless Mr. Coghlan wisely examines separately first the blacksmith's tools and methods of today, and then what are taken to be their forerunners in antiquity. Both author and reader are probably happiest in these sections which deal with the things which—of all that the book mentions—we can most easily do again. And in the last analysis we have here the most searching independent check on our hypotheses.

To a very large extent the artifacts carry their technological record unequivocally imprinted on their microscopic structure. It is therefore only right that pride of place should have been given in the book—as it is here—to the metallographic analyses carried out by Messrs. Stewarts and Lloyds. The results are presented in exemplary fashion and the micrographs are among the most beautiful I have seen.

In calling this section the book's most important, I am sure that I echo the author's feelings, for there is something definite about it that is always extremely satisfying. This in no way belittles Mr. Coghlan's efforts, either elsewhere in the book (where they are often patently prodigious) or indeed over the analyses themselves: I know from bitter experience how much work is involved, and not generally recognized, in the periods before and after such analyses.

Similarly, I am sure that the author will not mind my drawing attention to two cases where additional information has recently become available: it is a measure of the subject's vitality. First, the ring found at Býči Skála, Moravia (pp. 74, 78), must now be taken definitely not to have been made from cast iron. Secondly, Mr. J. W. Anstee (*Nature*, Vol. CLXXVIII, pp. 1430 ff., 29 December, 1956) has added considerably to our knowledge of pattern-welded swords.

Mr. Coghlan's book will take its place among the best now being produced in Europe in this field. It should be compulsory reading for all serious students of the 'Iron Age,' the millennia on each side of the birth of Christ. And it will for long remain an indispensable textbook to those of us who may not be quite sure, for instance, why spearheads and swords could not have been cast in iron as they were in bronze; or, particularly, how much information of value to the archaeologist can be obtained from a metallographic examination.

L. BIEK

**New Answers to the Fatigue Problem.** By Adelaide K. Bullen. Gainesville (U. of Florida P.), 1956. Pp. xii, 176. Price \$4.50

49

This book, written in a popular style, is an account of research on personnel in offices and factories undertaken by the Harvard Fatigue Laboratory. The approach is that of constitutional typology, a field with a long history which has recently enjoyed a recrudescence under the stimulus of Sheldon's writings. It is claimed by this school that differences in body build are significantly associated with differences in temperament. The author studied the performance and psychological attributes of workers engaged in various tasks in relation to their body build; the methods used in classifying the subjects into several categories of body build are not, however, discussed. Much of the book consists of fragments from the author's notes on the personalities of the subjects, and there is also some discussion of laboratory testing of performance.

The author's notes on her conversations with the workers have a human interest and might well be useful to those engaged in personnel management. As an education in scientific method for a wide public, however, the book seems to be thoroughly bad, since it gives no idea of the difficulties involved in research of this kind if conclusions which will stand up to criticism are to be reached.

N. A. BARNICOT

**Seven Caves: Archaeological Explorations in the Middle East.**

By Carleton S. Coon. London (Cape), 1957. Pp. 323, 24 plates, 36 text figs., 3 maps. Price £1 8s.

50

In MAN, 1957, 40, Professor Coon, reviewing H. Wendt's *I Looked For Adam*, refers to the 'science-gossipy' and inaccurate account of his 'fainting with excitement' at his 'discovery of Adam' contained in that book. In this book, on pp. 194f., I was easily able to recognize Professor Coon's own version of what happened on that occasion, even without the clue, which Wendt provides, that the locale was Hotu Cave, on the Caspian Sea. Quite apart from the spice that such a discovery lends to reading a book, this is an agreeably anecdotal account of the author's excavations in seven caves, ranging from Tangier to Iran, mostly off the beaten track, and covering a wide span of time from the Middle Palaeolithic down to the Neolithic.

This is not a technical book, as may be gathered from the extremely elementary introduction to Stone Age archaeology in Chapter I; on the other hand, the plans and sections and drawings of flints should combine with Professor Coon's gently instructive text to give the intelligent but ignorant reader a good idea of the techniques, finds and problems of cave excavation. If this was the aim of the book, then I consider that it has been achieved. The plates are excellent.

M. A. BENNET-CLARK

## ASIA

**The Scythians.** By Tamara Talbot Rice. London (Thames & Hudson), 1957. Pp. 255. Price £1 1s.

51

The volume before us is one of a series called 'Ancient Peoples and Places,' and it is stated of this series that 'each volume . . . will combine all the properties of an up-to-date archaeological textbook and a clear, readable account for the interested layman.' It may be said at once that this book is not an archaeological textbook in the usual sense of that phrase.

Various great people have written about the Scythians in the past. They are an extraordinarily interesting element in antiquity, and deserve the kind of study given to them by such people as Sir Ellis Minns, from whose work Mrs. Talbot Rice has taken much. In the present volume we find an addition to our equipment for future study in the illustrations, both photographs and drawings, many of which are of very well-known objects, frequently better illustrated than before. The limitations of price have no doubt to



bear their share of blame for the omission of illustrations of some important and significant pieces. The range of material brought under discussion is wide, and I should imagine that the parts of the book dealing with, for example, beards, or textile and leather work, would prove useful in providing a compendium of material not otherwise easily accessible. Unfortunately, the plan of the book, while most agreeably discursive, is vague, and the reader progresses from item to item in a disconnected way, without experiencing the feeling that he is getting to grips with his subject, or, indeed, that he ever will do so.

I personally dislike the dogmatism in which the book is steeped. For example: 'the princely burials which Kuftin excavated at Trialeti . . . produced quantities of gold and silver objects, many of which show definite signs of Hittite origin. Other finds of the same type . . . were found in Armenia. They must in consequence have been imported direct from Asia Minor' (p. 150). This kind of fantasy is not rare in the book, and the serious student is likely to object, while the 'interested layman' is only too likely to get a false impression. If our author had wished to enter upon such a line of thought she ought to have given chapter and verse for such startling statements, and provided illustrations of her alleged 'Hittite' parallels.

The drawing of a picture of any of the peoples of antiquity, and perhaps especially of those, like the Scythians, who seem never to have settled for very long anywhere, is of absorbing interest, and extraordinary difficulty. Under such conditions it is in the highest degree unfortunate that attempts should be made to form conclusions about origins, trade and so on unless full documentation is provided. Personal opinions are certainly always of interest, however dogmatically expressed, and the present volume is no exception, but unless care is taken to warn the non-specialist reader when they, rather than facts, are being quoted, reviewers are likely to criticize.

T. BURTON-BROWN

#### An Anthropological Reconnaissance in the Near East, 1950.

By Henry Field. Cambridge, Mass. (Pap. Peabody Mus., Vol. XLVIII, No. 2), 1956. Pp. x, 119

52

Yet another publication by Henry Field adds to the wealth of miscellanea collected by him in the Near East. Data included are ABO blood groups of 58 Assyrians in Iraq; measurements of large samples of Assyrian male levies, females (previously published), Bakhtiari tribesmen, and of smaller groups of labourers at Nippur (mostly Shergatis), Lurs, Kurds and Beharna; lists by various authors of identified plants, bats, birds and cave fauna; and unexplained tables concerning dust and rainfall in Baghdad and a few other places. The whole is presented in Dr. Field's characteristic manner.

D. F. ROBERTS

#### Studies in Indonesian Archaeology. By Dr. W. F. Stutterheim.

Koninkl. Inst. voor Taal-, Land- en Volkenkunde, Translation Series. The Hague (Nijhoff), 1956. Pp. 158, 23 plates. Price 14 guilders

53

This first volume of a new series of translations of important Dutch publications, dealing with research in Indonesian culture, contains a bibliography of the works of the late Dr. W. F. Stutterheim and the English versions of five of his papers. These are considered below.

'Chandi Barabudur; Name, Form and Meaning' (1929, pp. 62) is an inquiry into various problems connected with the famous stupa-sanctuary of Mahāyāna Buddhism in Central Java. Whilst Krom and van Erp, the authors of the great monograph on the Barabudur, considered the Hindus to be the main designers and builders of this monument, Stutterheim emphasizes the part played by the Javanese. He expresses the opinion 'that the system of the Barabudur is the same as that of the older parts of the Sang Hyang Kamahāyānikan, a Javanese treatise on the Mahāyāna, greatly different from the Indian view.' He stresses the need for a new edition of this work, which he regards as a major source of information for studies in Javanese culture.

'Some Remarks on Pre-Hinduistic Burial Customs on Java' (1939, p. 26) deals with certain features of the Indonesian death cult during Hinduistic times which point to the survival of a non-Indian ancestor-worship. Stutterheim's view that South-West China was

the cradle of the early civilizations of the Archipelago is today generally accepted.

'A Thousand Years Old Profession in the Princely Courts on Java' (1935, p. 9) is an iconographic study on certain Barabudur reliefs, in which the figure of a bearded brahman appears among musicians and dancing girls. Stutterheim shows that this character plays the role of a court jester or master of ceremonies—a profession which has survived to the present day in parts of Middle Java.

'An Ancient Javanese Bhima cult' (1935, p. 37) is a comparative study of the so-called Bhima statues of Java and their Balinese counterparts, demonstrating the existence of a Bhima cult on Java during the late Majapahit period. The author urges further research into Javanese literature to determine more closely the nature of this cult.

'An Important Hindu-Javanese Drawing on Copper' (1924, p. 12) describes a unique representation of a woman carrying a child, distantly related in style to the Ajanta figures, and surrounded by an inscription which indicates the religious character of the drawing; it is of the eighth or ninth century.

It is impossible in this short review to do justice to Stutterheim's profound knowledge, originality and wealth of ideas, which are a never ceasing source of inspiration to the student of Indonesian arts and letters.

PRINCE JOHN LOEWENSTEIN

#### Federation Museums Journal, Vols. I & II, New Series, 1954-55. Federation of Malaya (Museums Department). Pp. 143.

54

Price £1 1s.

The *Journal of the Federated Malay States Museums* stopped publication in 1941. Now, under a new name dictated by the change in Malayan government, it reappears at last. The Editor, Mr. G. de G. Sieveking, Curator of Museums in the Federation, has brought together a valuable collection of papers falling within the fields of anthropology, ethnology, and archaeology, the subjects to which the *Journal* will be devoted. (Zoological papers will be published in the *Bulletin of the Raffles Museum*.)

Malayan Aboriginal cultures account for three of the papers in this new number. Mr. R. O. D. Noone writes on the trade in blowpipes and blowpipe bamboo in North Malaya, Mr. J. A. R. Blacking on musical instruments in Malayan museums, and the late P. D. R. Williams-Hunt on a Lanoh Negrito funeral. Mr. H. H. Beamish has contributed a paper on 'The Animals on Ming Trade Porcelain.' Mr. D. Walker writes on 'Alluvial Deposits of Perak and Changes in the Relative Levels of Land and Sea' (which is the first part of 'Studies in the Quaternary of the Malay Peninsula'), while the Editor contributes Part I of 'Excavations at Gua Cha, Kelantan, 1954' and a Note on the distribution of wild species of rice. The *Journal* is very well illustrated and beautifully printed (at Cambridge). Future volumes will be awaited with interest.

MAURICE FREEDMAN

#### The Religion of the Bare'e-speaking Toradja of Central Celebes. By Richard Erskine Downs. The Hague

(Excelsior), 1956. Pp. viii, 107, bibl.

55

The principal source on the religion of the Bare'e-speaking Toradja is the monumental but confused (and confusing) work of the Dutch missionaries Adriani and Kruyt: *De Bare'esprekende Toradja's* (3 vols., 1912). In the work under review (for which the author was awarded a doctorate at Leiden University) Dr. Downs sets himself the task of assembling the data on religion, scattered through the first two volumes of Adriani and Kruyt's work, and presenting them in a coherent fashion. In doing so, he has to explain the internal inconsistencies, and the discrepancies between the first edition of 1912 and the second of 1950-51.

This leads, e.g., to a useful clarification of the distinction between 'werewolves', 'witches' and 'sorcerers' in this religion (p. 39), and to the conclusion that when modern publications (such as the 1951 edition of *Notes and Queries on Anthropology*, p. 176) speak of the belief in 'soul substance' in Indonesian and Melanesian religions, this is largely due to the influence of Kruyt's earlier works, and a neglect of his later and sounder views (p. 36).

Where Downs surmises missionary influence as a cause of the Toradja belief in cleansed souls going to the afterworld (p. 88), I



think that he is in error through having overlooked the passage in Vol. I, p. 130, of the first edition.

More important from a scholarly point of view than the useful but humdrum work of orderly compilation is the way in which the author tests the applicability of some modern theories by means of the Toradja data: Kluckhohn and Leighton, writing on Navaho religion, and Nadel, on the Nupe, attempted to explain the social role of witchcraft, and, in the latter case, why it was attributed predominantly to women. Do these explanations hold good for the Toradja (pp. 42-44)?

The main value of this book lies in the way in which the various elaborate rites and myths of the Toradja are shown to derive from a basic view of the universe, and man's part in it. So we are helped to understand the striking similarity between the girls' initiation feast, the headhunting ritual and myths, and the funerary ceremonies. The ideal man is a headhunter, the ideal woman a shaman. The *rites de passage* of headhunting and female initiation are accomplished by the assistance of the ancestors in the underworld, in the case of the headhunters, and of the good spirits of the upperworld, in the case of

the shaman. Together, the lower and upper worlds maintain the cycle of death and rebirth, the orderly functioning of the cosmos. The funerary ritual, another *rite de passage* for the deceased individual, is part of this cycle for the community as a whole. So is the rice harvest, at which the rice is sacrifically killed with the aid of 'practically all the gods, spirits, and ancestors' of both worlds.

This theme, in my opinion most important for an understanding of Indonesian religions, is, rather unfortunately, developed with undue diffidence, and almost hidden away in the form of summaries at the end of various chapters (pp. 44, 67, 75, 91, 98-100). These summaries are so concise that they imply more than they state, and many a reader might overlook their significance.

If Dr. Downs will, in a later publication, develop the highly promising line of thought of this book, expanding it, and taking into account the data from the Indonesian regions which he discussed in an earlier paper (*Bijdragen Koninklijk Instituut*, Vol. CXI, p. 40 ff.), the result will undoubtedly be a work of major importance for the study of 'primitive' religions, in Indonesia and elsewhere.

P. E. DE JOSSELINE DE JONG

## EUROPE

**L'Arte dell' antica età della pietra.** By Paolo Graziosi. Florence (Sansoni), 1956. Pp. xi, 276, 300 plates. Price Lire 14,500

56

One's first reaction on seeing yet another publication on palaeolithic art is of slight alarm. Such volumes are generally large, very expensive and frequently merely reproduce illustrations which are all too familiar. Professor Graziosi's book is larger, heavier (8 lb.) and more expensive (about £8 15s.) than most, and is only justified if it furthers our knowledge of the subject, represents it more fully and clearly or produces a complete and usable reference book. Even a cursory glance is sufficient to show that this is indeed a considerable advance on the publication standards of its predecessors and goes a long way towards meeting the prerequisites mentioned above.

The text includes a full bibliography, there are two maps, and of the plates, several of them in colour, many are multiple, so that the number of actual illustrations is considerably in excess of the number of plates.

The ground covered is precisely what the title states and all aspects of palaeolithic art are treated, the first part of the plates dealing with mobile art and the second with parietal; this overall treatment makes the book of considerable value for reference. The mobile art is divided into sections, bone tools, figurines, engravings on stone, etc., and though some of the illustrations are rather poor the general standard is extremely high. Many of the photographs are accompanied by line drawings which are a very great help, though some of these are rather inaccurate when compared with the originals alongside.

The second part of the plates covers the parietal art, including sculpture and modelling in clay. Naturally the greater part is devoted to the Franco-Cantabrian region, but there is a section covering 'La Provincia Mediterranea' which includes La Pileta, La Baume Latrone, Ebbou and Italy down to Lavanço; there is no mention of the Eastern Spanish art group. As with the first part the standard of the illustration is extremely high with Lascaux, Font de Gaume and Altamira taking most of the coloured plates, though there are some very pleasant illustrations from La Pasiega. Where engravings were used with the paintings these are often shown separately below—a very useful feature.

So far we have only dealt with the illustrations; the text, however, occupies nearly half of the book. It begins with a rather slight introduction to the chronology, human types and industries of the Upper Palaeolithic, followed by sections on technical development, etc., and descriptions of the plates. The sub-division of the Upper Palaeolithic into Aurignacian-Perigordian, Solutrian, Lower and Upper Magdalenian seems to be rather an over-simplification, as much of the mobile art at least is dated to within finer limits than this. The final part of the text consists of an excellent bibliography, index and two distribution maps, one for the mobile and one for the parietal art.

To sum up, one can say that Graziosi has given us a book of excellent quality which will prove to be the reference book *par excellence* for many years to come and both he and the publishers are to be congratulated on producing a book which prehistorians are strongly recommended to buy.

J. WAECHTER

**Prehistoric Man in Denmark, Vols. I and II.** By K. Brøste, J. B.

57

Jørgensen, C. J. Becker and J. Brøndsted. Copenhagen (Munksgaard), 1956. Vol. I, pp. 159, 46 text figs.; Vol. II, pp. 439, 265 plates. Price 230 Danish kroner

Dr. Johannes Brøndsted, the Director of the National Museum at Copenhagen, makes the aim of this publication clear in his preface. It is intended to make generally available what data on the prehistoric physical anthropology of Denmark's present area now exist. The thing was begun in 1937—the year before the First International Congress of Anthropological and Ethnological Sciences in Copenhagen—; it was planned by Dr. Brøndsted and by two men now dead, Professor Hou-Jensen and Dr. Fischer-Møller, both of whom many of us will remember for their enthusiasm and courtesy. War and occupation did not stop the work, nor were the repeated strokes of death allowed to halt it. A grant from the Rask-Ørsted Foundation made possible the translation of the text into English by Mr. W. E. Calvert, and the Carlsberg Foundation gave generous subventions towards the preparation and printing.

The two volumes now available cover the mesolithic, neolithic and Early Bronze Age finds, though not (it appears) the stone-age material amassed since 1944. The post-bronze and remaining stone-age finds will be dealt with in the next volume. Every effort has been made to correlate each specimen described with its proper archaeological level, all possible criteria, including pollen-analysis, having been used. Indeed, only such finds as have been so correlated are described. Anthropometrical technique, statistics, Scandinavian comparisons and conclusions are given in the first volume. The second volume is a magnificent picture gallery of half-size photographs of crania in as many of the four *normæ* as each skull allowed of, together with the corresponding craniograms *secundum Sarasin*. The first volume also contains clear and informative historical and archaeological introductions.

Crania, long limb bones and pelves are dealt with separately. The crania are datable as: mesolithic (4), early neolithic (4), middle neolithic (47), late neolithic (72), middle or late neolithic (133) and Early Bronze Age (5): a similar classification was made of some 856 long bones, whose numbers in each group showed proportions of the whole not unlike those of the crania. It was impossible, however, to assign long bones to more than 21 crania. Only 23 pelves were usable. From a thorough and commendably careful analysis and statistical synthesis of this material, checked by the *t* test, the authors



conclude that stone-age man in Denmark increased in size but did not change in shape as the centuries rolled on, this being true for both men and women. They found no reason for thinking that the arrival of the Single-Grave-Culture folk made any difference to physical form. From the Maglemose to the Bronze Age periods there was an increase in height from an average of 1650 to one of 1750 mm.—from low medium to high medium stature. Throughout they had large skulls, somewhat high and of medium breadth; with broad to medium width of face and shortish palates.

These two volumes are a credit to the publishers. They are not indexed, however, and reference from part to part is difficult. The page is large (12 x 6 in.) and easily read. Because of the complete and authoritative nature of this work it should be bought by every anthropological, archaeological and general reference library, and it could well be used as a model text by the senior student of skeleto-metry.

M. A. MACCONAILL

**Excavations at Jarlshof, Shetland.** By J. R. C. Hamilton. London (H.M. Stat. Off.), 1956. Pp. xiv, 228, 40 plates. Price £3 3s.

58

Jarlshof is situated on a small headland in the West Voe of Sumburgh at the southernmost tip of Shetland. The site received its name from Sir Walter Scott, but Mr. Mertoun's tenancy of the tumbledown house was preceded by an occupation which went back to the neolithic and Early Bronze Age. Excavation on the site was first undertaken by Mr. Bruce towards the end of the last century and was continued by a distinguished succession of scholars—Dr. Curle, Professor Childe, Dr. Richardson, and latterly Mr. Hamilton, who now publishes the result of all these excavations in a fine volume which has the grave disadvantage of being printed on the vilest type of Stationery Office paper, a feature that detracts greatly from the appearance and feel of the book. But nothing could detract from the value of Mr. Hamilton's work: with admirable clarity he presents the results of the excavation layer by layer, starting with an amorphous Early Bronze Age/neolithic level, progressing through a well built Late Bronze Age village, a series of Early Iron Age houses, to a broch and a post-broch settlement which seems to have lasted continuously until Stuart times. The site shows a remarkable continuity of settlement and economy. Its good harbourage and valuable grazing have always attracted settlers; the steatite industry of the settlement, an important feature throughout its history, emphasizes, perhaps more than anything else on the site, the continuity of the economy. This continuity is reflected in the uniform economic standard presented by the site; the buildings, so solidly and lavishly built, and the pottery, though different in appearance from period to period, uninterestingly uniform in quality.

The excavation was carried out, sometimes under the most appalling meteorological conditions, with a care and skill which produced not only a large amount of archaeological material but also an interesting and instructive preserved monument, which is now under the guardianship of the Ministry of Works. The technical results of this excavation, as shown in this book, are of admirable quality; the plans are clear and to the point and the small drawings reconstructing the different stages of the settlement help us to appreciate the value of the multifarious lines drawn on the plan.

One might quarrel with the presentation of the finds. It is perhaps rather disturbing to the reader to find that the descriptions of two successive stages of construction of the same house are divided from each other by a long and complicated list of finds. Could not the finds have been treated separately at the end or at the beginning of each chapter, with the main description, discussion and conclusions placed in one continuous piece of prose? It is also rather disturbing that the numbers given to the objects on the site are emphasized by the use of a bold type face and yet have no meaning within the book. The report on the animal bones is of poor quality; there has obviously been a certain amount of trouble in its production, but one would perhaps have appreciated a general summary on the utilization of the different animal bones during the separate periods; it is surely insufficient to say that there appear to have been two sizes of ox

during the Stone Age, without some sort of proportional statement and without some hint at least of the comparative sizes of the two beasts. But, with the exception of the latter point, these are minor criticisms. The implications of this report will be of great value to social and economic historians for many years to come and the chronological evidence obtained from the site will provide a crutch on which future generations of Scottish archaeologists will lean heavily.

DAVID M. WILSON

**Lapska Offerplatsfynd från Järnålder och Medeltid i de Svenska Lappmarkerna.** By Inga Serning. *Acta Lapponica* XI. Stockholm, 1956. Pp. 222, 65 plates, 16 text figs., 4 tables. Price 75 Sw. crs.

59

It is surprising how little is known with certainty about the Lapps of even 400 or 500 years back. This depends on the scarcity of data and one has to be careful to avoid unwarranted generalizations from the few facts available. The conditions of a people so closely depending on nature may vary as greatly as the geographical environment and climate in Fennoscandia, in both space and time. The change from fishing and hunting to an economy based on reindeer-herding with large units may, for instance, have been very uneven. Looking back even further one can construct the past of the Lapps only from the evidence of archaeological finds and place names.

Every factual contribution to the study of Lappish prehistory must be welcomed. Inga Serning has in her doctor's thesis made available for research and investigation finds which have been collected by Dr. Gustaf Hallström and others. The finds concerned are from Lapp places of sacrifice in Sweden and date from the Iron Age and Middle Ages, from the period A.D. 1000-1200. These are the oldest known finds. In many cases the dating of the objects is uncertain, but the coins aid in the estimation. These are analysed in a numismatic appendix by the experts Ulla S. Linder Welin (Arabic coins), Vera Jammer (German and Polish coins), Brita Malmer (English, Danish and Norwegian coins) and Nils Ludvig Rasmussen (coins from the period after c. 1100).

The main part of Serning's work consists of systematic series of descriptions of the objects and catalogues of the finds from each of the 11 places. They are all north of Lat. 64°; the Lule River area accounts for most, and one site is even on the Norwegian frontier. The objects are described in detail and photographic reproductions, mostly at actual size, are given of almost all, so that the initiate reader can independently make his conclusions. In this way the work offers a safe basis for further research and fits well into the *Acta Lapponica* series (edited by Ernst Manker), of the Nordiska Museum at Stockholm, which has spared no cost in presenting the whole volume in splendid layout and clarity. The objects consist of buckles, rings, earrings, necklaces, beads, buttons, dress decorations, swords, arrowheads, harpoons, knives, needlecases, locks and keys, and coins. Their variety and richness catches the eye. The investigation of the coins has shown it to be probable that they came into Lappish hands during the early and middle twelfth century. Thereafter the intake of coins seems to have diminished and they are almost confined to Norwegian coins, of a kind hardly to be found in Sweden, and indicating a direct import from the west.

In a special chapter the author has related the finds to the historical and cultural background; they naturally throw light on the connexions and contacts in the past. She emphasizes the wealth of the non-Lappish items, showing a measure of prosperity, and remarks that the finds from later places of sacrifice reveal in contrast only bone and horn remains. She passes over the question of whether there were changes in the manner of cult and worship; and starting from the fact that the earliest Lappish graves on the Arctic coast are also without foreign traces from times prior to the period treated, she concludes that such items did not come into Lappish hands before the twelfth century. The later diminution in the incidence of those objects is related to the development of the trade centres from which agents came to purchase furs and hides from the Lapps. This interesting chapter is given almost *in extenso* in the English summary which is included.

K. NICKUL

35196



**Lapland.** By Johannes Schefferus, translated from the Latin by Henrik Sundin, John Granlund and Bengt Löw, and edited by Ernst Manker, with John Granlund, Bengt Löw and John Bernström. Nordiska Museet, Acta Lapponica VIII. Uppsala, 1956. Pp. 472, illus. Price 35 Sw. crs.

**60** *Lapponia* was published in Latin at Frankfurt in 1673, in English at Oxford in 1674 and again in 1704 as *The History of Lapland*, in German at Frankfurt in 1675, in French at Paris in 1678, and in Dutch at Amsterdam in 1682, and remained for long a 'classic' or 'standard work' on Lapland. It is essentially an early, painstaking compilation of a number of historical and contemporary sources. This first Swedish translation and edition is belated by nearly 300 years.

Through the joint efforts of several specialists, this collectors' piece is now presented as an edited ethnographical treatise in modern format and orthography, in a language of neighbours of the Lapps—one that most Lapps and students of their culture understand. The original Latin edition included many important excerpts from Swedish informants; these were omitted in the English translation of 1674 and the value of the work seriously diminished thereby. In this edition the whole of the original is presented in one language; Schefferus's supplement (F.e 7) to the main work is also included for the first time, both in translation in the text and in the original Latin in the Notes following. The Swedish edition is also fortunate in its Introduction, 'Johannes Schefferus och hans Lapponia,' by Bengt Löw. This, together with background information in Collinder's book *The Lapps* (Princeton University Press, 1949), usefully interrelates the author, his subject matter and European interest in it.

Queen Christina of Sweden (who invited Descartes to teach her philosophy, and so to his death bed) was responsible for the engagement of the Alsatian philologist Johannes Schefferus as professor of Law and Rhetoric at Uppsala; and it was a Lord High Chancellor of Sweden, Magnus de la Gardie, who directed the professor to Lappish studies in order that this people should be made known to the outside world, especially their religious and magical beliefs and practices. The enemies of Sweden, in apparent exasperation, had attributed her outstanding military successes to the magic devised before battle by Lapps wilfully employed by Gustavus Adolphus. Schefferus wishes dutifully to discredit this seditious notion, of course; yet much of the attention given to his book abroad suggests that the notion was instead given new vigour. Nor am I particularly convinced by the present editors' claim that Schefferus's work brought enlightenment and realism into the foreigners' view of the Lapps. The 1674 edition for the foreigners in Oxford was prefaced with '... but here it is indeed, where, rather than in America, we have a new world discovered.' The 'enlightenment' today has perhaps reached the stage at which most tourists find something disappointing about those Lapps that they see.

It appears that Schefferus completed his MS. within two years. This was only made possible through the commands of his Lord Chancellor to various clergy in Lapland that they were to provide the scholar—who remained at Uppsala, in his library and with his students, some few of whom were Lapps—with detailed descriptions concerning the inhabitants of their Lappish parishes. In addition to these short field monographs as they are recognized today, two of whose authors were Lapps, Schefferus had at his disposal the recently (1670) published political and economic treatise of one Johan Graan, a provincial governor in Swedish Lapland, who was of Lappish origin. Schefferus was familiar also with classical and medieval, Latin and Nordic histories, and *Lapponia* is enhanced and strengthened both as literature and as ethnography with the observations and speculations of Virgil and Tacitus, Orosius and Procopius, Adam of Bremen and Saxo Grammaticus, Marco Polo, Jakob Ziegler, Olaus and Johannes Magnus, Peder Claussön Friis and others. In his 35 chapters, Schefferus is always relevant and exemplary in the critical editing and assessing of his varied material.

What the histories had to tell of 'Fenni,' 'Skridfinnia,' 'Terfinnia' and 'Biarmia,' 'Lappi,' 'Loppi' and even 'Pilappii' (Peucer)—all appellations for the people who called themselves by variants of the term *Sabme* (pl. *samek*)—perhaps adds up to little; quite the opposite

is the case with the first-hand reports which Schefferus received from the clergy in Swedish Lapland. Formally at least there is a long line of historical continuity, such as is unapproached in either Eskimo or Siberian arctic studies, and it is for its systematic embodiment in *Lapponia* (or *Lapland*) that Lappologists will long continue to turn to the book.

ROBERT PAINE

**Arctica: Essays presented to Åke Campbell, 1. V. 1956.** Edited by A. Furumark, S. Lagercrantz, I. Ruong, D. Strömbäck and G. Widengren. Stud. Ethnog. Upsal., Vol. XI. Uppsala, 1956. Pp. ix, 296. Price £6

**61** This volume was issued in honour of Åke Campbell on his sixty-fifth birthday. In Great Britain Professor Campbell is perhaps best known as author of articles on Irish material culture; in Sweden he is renowned for his books and articles on the Swedish use of bread and for his great study of culture contact in Lapland (1948). The work here in consideration presents a true picture of the multifaceted interests of the distinguished scholar.

The majority of the articles deal with Irish and Lapp culture. Thus, in the field of Irish ethnology we find a presentation of Irish farmyard types by C. Ó. Danachair, a discussion of the distribution of wattle and straw mat doors in Ireland by A. T. Lucas, some notes on death customs in Ireland by Ó. Súilleabháin, and an analysis of the ecclesiastical background of the Irish saga literature by J. Carney. Among the contributors on Lapp culture are K. Vilks, who writes on earlier Lapp covers on their *kotas*, I. Ruong, who describes the Lapp types of husbandry and their relation with the settlement types, Gutorm Gjessing, who writes on inter-Lapp culture contact in Norway, and Dag Strömbäck, who compares Lapp and old Swedish pictures (on Gotland) of the realm of death. The old culture stratum of the Arctic and Subarctic is touched upon by A. Eskeröd and S. Lagercrantz, and K. Nickul, I. Whitaker, and J. Granlund analyse different forms of social process. The volume is introduced with a short statement on national character by B. Collinder.

In a short review it is impossible to give a fair treatment of all the many and different topics represented in this volume. Let us, therefore, select for further discussion a couple of the articles at random. Collinder faces the difficult problem: what is national character? He states that the average mental attitude is different in different countries: 'people do not react in the same way to the same stimuli all over the world.' In explaining this fact he first emphasizes that the tendency to disregard the biological factors is too strong in our time: 'it is not true, from a qualitative point of view, that all men are born alike.' On the whole, however, he considers 'that the character of a people usually emanates from a social stratum that sets the fashion, spreading their conceptions and values, 'the leading ideas.' Collinder's points of view agree well with those of most contemporary anthropologists in Scandinavia. In his article on 'Declining Transhumance as an Index of Culture Change,' Whitaker, like Malinowski, stresses the importance of observing the dynamic nature of culture change, in other words, acculturation (in the American sense). He tries to elucidate such a process in the case of the mountain Lapps, whose aboriginal way of life has been modified through contact with the settlers. This modification shows in the transhumance cycle (the author prefers to characterize Lapp migratory movement as transhumance—a debatable point!). This cycle has, according to the author, 'declined'; only the active herders follow the reindeer to the extreme points of the migration routes. We have thus in Lapland a third cultural reality, identical neither with the old Lapp culture nor with the Swedish culture in the neighbourhood. Professor Granlund's instructive paper on the change of a Swedish community from a settlement of fishermen to a town of burghers should be mentioned as a brilliant instance of modern Swedish ethnological research on culture process. Finally, the chief editor of the volume, Dr. Lagercrantz, has written a detailed and learned article on the distribution of 'Some Eurasian Release Systems.' He does not confine himself to the Eurasian area, however, but discusses also African and North American data.

All in all, this is an interesting volume, worth reading. The typography is excellent.

ÅKE HULTKRANTZ





(a) The shelter from the north, showing the abrupt edge to the deposit, and the positions of the two trenches



(b) Looking north-west from the shelter



(c) Trench B from the opposite side of the torrent bed



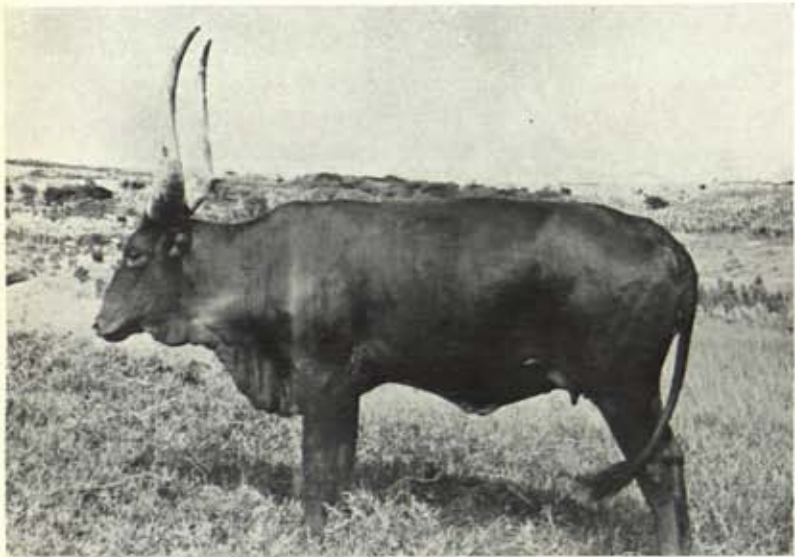
(d) Looking across to the open country at Marmar el Burqa, from the shelter. Note the vertical face of the rock on the opposite side of the torrent bed.

**THE MADAMAGH ROCK SHELTER, NEAR PETRA, JORDAN**

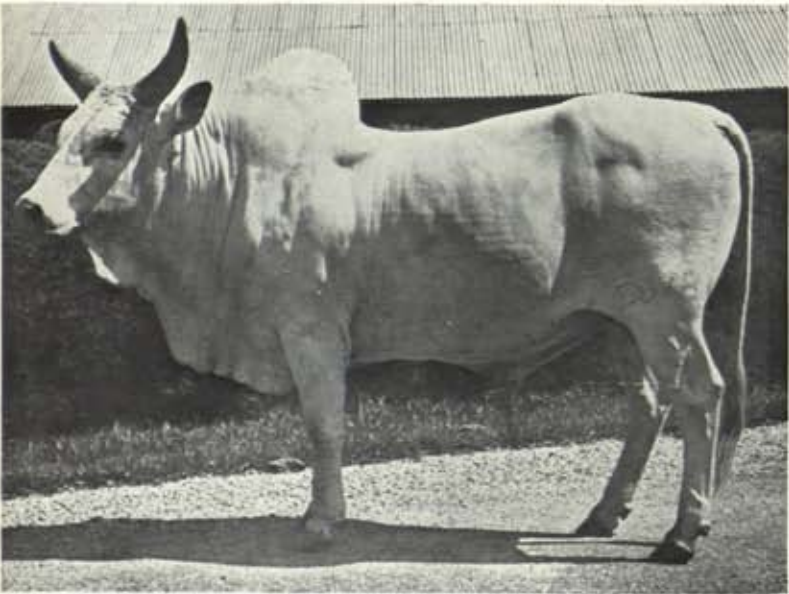




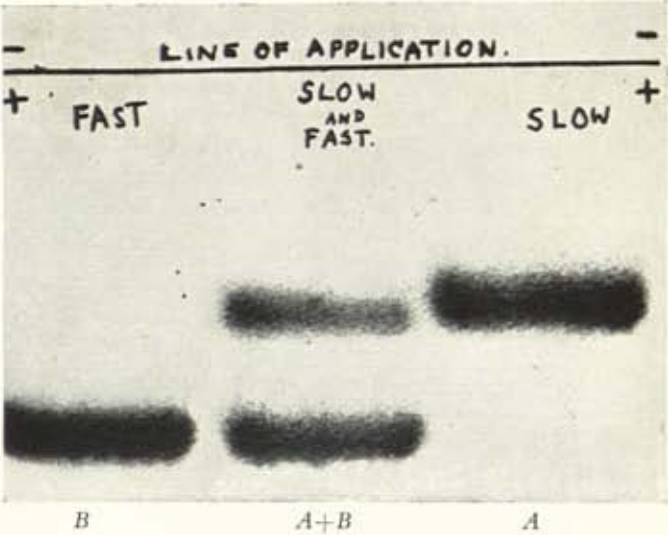
(a) A herd of Ankole cattle



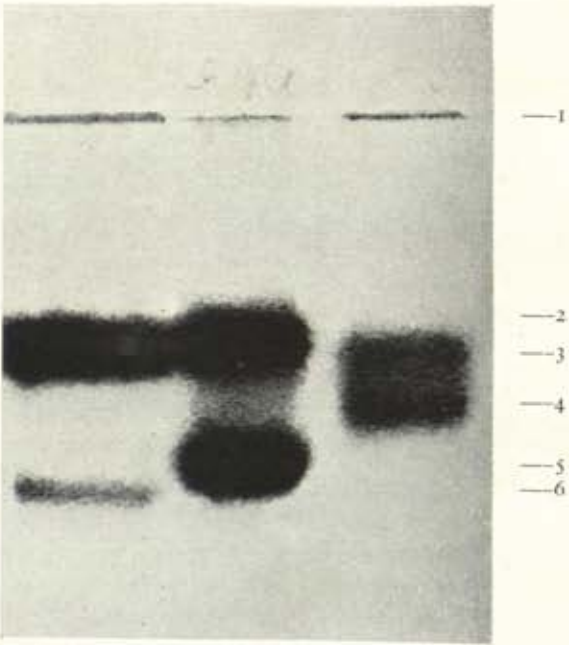
(b) An Ankole cow



(c) A Zebu bull



(d) The three haemoglobin phenotypes of adult cattle (paper electrophoresis at pH 8.6)



(e) Comparison of the two cattle haemoglobins with human haemoglobins (paper electrophoresis at pH 8.6). 1, line of application. Cattle haemoglobin A (2) moves just a little less fast than human haemoglobin A (3); cattle haemoglobin B (5) moves between human haemoglobins H (6) and J (4).

CATTLE AND THEIR HÆMOGLOBINS IN UGANDA



# THE HÆMOGLOBINS OF 211 CATTLE IN UGANDA\*

by

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and

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**62** In Uganda there are two main types of cattle: the long-horned Ankole in the western province and the short-horned Zebu in the northern and eastern provinces. These types differ in conformation, in the anatomy of the hump (Milne, 1955), and in their resistance to tuberculosis (Carmichael, 1939).

The Ankole (called sometimes Sanga) is thought to have come into being 3,000 to 4,000 years ago when the lateral-horned Zebu was brought to Africa from Asia and was crossed with the long-horned African cattle then owned by the Hamites (Curson and Thornton, 1936). It is especially associated with the Ankole district but is also found in the neighbouring districts of Uganda, in Ruanda-Urundi, and in north-western Tanganyika. The owners of the Ankole cattle are principally the Hima, who are classed as Hamitomorphs (Oschinsky, 1954).



FIG. 1. A NGANDA COW

*The absence of the horns occurs in some of them naturally. The animal resembles in some respects (notably the hump) the Ankole, and in others, (colour, dewlap, size of udder) the Zebu.*

The short-horned Zebu is a mixture of the lateral-horned Zebu and the Asiatic *Brachyceros*. This cross breed is thought to have followed the pure lateral-horned Zebu from Asia into Africa about 2,000 years later (Curson and Thornton, 1936). Compared with the Ankole, the short-horned Zebu is therefore to be considered as being of Asiatic rather than Asiatic-African stock. The owners of the short-horned Zebu in Uganda are the Nilo-Hamito-

morphs such as the Teso and the Karamajong who live in the north-east of the Protectorate (see Oschinsky, 1954).

Mixtures of the Ankole and of the short-horned Zebu are found in the regions where the two types of cattle meet. Such a cross breed is the Nganda of central Uganda, a definite but as yet unfixed type.

It is known that adult cattle possess two types of hæmoglobin which can be differentiated by electrophoresis (Cabannes and Serain, 1955). On paper electrophoresis at pH 8.6 one moves a little more slowly than the normal hæmoglobin of adult man and the other moves faster. The slow hæmoglobin has been called Hb.  $\alpha$  by Cabannes and Serain (1955) or Bov. A by Bangham (1957) and the fast hæmoglobin Hb.  $\beta$  or Bov. B. Cabannes and Serain (1955) found in Algiers that some of the cattle possess both hæmoglobins and some have only one or the other. Bangham (1957) presented evidence that the hæmoglobins are inherited as if they were controlled by two allelomorphous genes, neither of which shows dominance or recessivity, both being recognizable in the heterozygote. He also showed a racial variation in the incidence of the phenotypes. Most cattle in Britain are homozygous for the gene responsible for Bov. A ( $\alpha$ ), but in Jersey, Guernsey and South Devon breeds a proportion of animals were found to possess Bov. B ( $\beta$ ). Bangham related this finding to the possible Indo-African origin of the Jersey breed. Indeed this would help to explain the known ability of the Jersey cattle to thrive in adverse tropical conditions (Stapleton, 1953).

Both Bangham, and Cabannes and Serain (1957), also found a third hæmoglobin in cattle which is present only in young calves. It migrates on electrophoresis between the two adult bovine hæmoglobins. This third cattle hæmoglobin is considered to correspond to the foetal hæmoglobin of man.

Uganda is remarkable for the difference in the distributions of normal adult and sickle-cell hæmoglobin amongst its communities (Lehmann and Raper, 1949). We decided to investigate, therefore, whether differences in the hæmoglobin distribution could also be demonstrated between the two principal types of cattle. We were particularly interested in the possibility of there being a balanced polymorphism such as has been found to exist in man for the genes responsible for normal adult and sickle-cell hæmoglobin.

We have examined altogether 211 head of cattle above the age of three months. The age limit was chosen to avoid difficulties in interpretation of the hæmoglobin pattern

\* With Plate F, a text figure and three tables



TABLE I. BOVINE HÆMOGLOBIN PHENOTYPES IN 211 CATTLE IN UGANDA

Cattle			Hæmoglobin		
Type	Number		A	A+B	B
Ankole .. ..	76		46	30	0
Nganda .. ..	33		20	11	2
Nganda × Zebu ..	26		17	8	1
Zebu .. ..	76		38	27	11

TABLE II. HÆMOGLOBIN-GENE FREQUENCIES IN 211 CATTLE IN UGANDA

Cattle			Gene Frequencies	
Type	Number		A	B
Ankole .. ..	76		0.8026	0.1974
Nganda .. ..	33		0.7727	0.2273
Nganda × Zebu ..	26		0.8077	0.1923
Zebu .. ..	76		0.6776	0.3224

which might possibly be caused by the presence of foetal hæmoglobin in the blood of young calves. The cattle came from different herds: 76 Ankole from two, 76 short-horned Zebu from four, 33 Nganda from two and 26 Nganda—short-horned-Zebu cross breeds from one. Not all the cattle from a particular source had belonged originally to that herd and some had been acquired from other herds. Table I shows the distribution of the hæmoglobin phenotypes in the four different types. It will be seen that both cattle hæmoglobins occur in all the types but that the Ankole cattle possess more genes for hæmoglobin A than the Zebu cattle. Table II shows the gene frequencies in the four types and Table III shows the number of phenotypes expected and actually observed.

As both hæmoglobins occur in the four different populations it is permissible to combine them, to determine the gene frequencies for the whole sample and to compare the number of phenotypes expected and found. It will be seen in Table III that there is a fairly good agreement between the distribution of the phenotypes expected and observed and that there is no obvious evidence for a balanced polymorphism or indeed any other process of natural selection which could be compared with that acting on the distribution of the human hæmoglobin phenotypes in Uganda (Allison, 1954; Raper, 1955; Lehmann and Raper, 1956).

There is, however, one parallel with man which may be noted. In man the newborn shows a comparative immunity against malaria which disappears at about the same time at which the foetal hæmoglobin ceases to be a significant component in the infant's blood (Lehmann, 1953; Allison, 1954). A similar position might occur in cattle since it is known that an infection with East Coast fever is much more likely to be fatal in adult cattle than in

TABLE III. DISTRIBUTION OF HÆMOGLOBIN PHENOTYPES IN 211 CATTLE IN UGANDA

Cattle		Number of Phenotypes					
Type	Number	Observed	Expected	Observed	Expected	Observed	Expected
Ankole .. ..	76	46	48.96	30	24.08	0	2.96
Nganda .. ..	33	20	19.70	11	11.59	2	1.71
Nganda × Zebu ..	26*	17	—	8	—	1	—
Zebu .. ..	76	38	34.89	27	33.21	11	7.90
All breeds ..	211	121	119.83	76	78.36	14	12.81

\* Number of sample too small to allow calculation of expected frequencies of phenotypes

young calves, and that the relative immunity of the calves is not obtained from the mother *via* the colostrum (Barnett, 1956).

Both the Ankole and the short-horned Zebu are partially derived from the Asiatic lateral-horned Zebu but the Ankole is considered as having inherited a large proportion of genes from its African ancestor, the long-horned humpless cattle of the Hamites. It does not seem unreasonable to conclude that the higher incidence of hæmoglobin Bov. A ( $\alpha$ ) in the Ankole is an expression of its African rather than its Asiatic inheritance.

#### Acknowledgment

We should like to thank Dr. Ada Kopeč of the Nuffield Blood-group Centre for advice and help in preparing Table III.

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# A KEBARAN ROCK SHELTER IN WADI MADAMAGH, NEAR PETRA, JORDAN\*

by

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London

**63** A few finds of flint artifacts in Petra were collected and recorded by Murray and Ellis (1940) and Kennedy (1925) between 20 and 30 years ago. While engaged on excavations in the city of Petra on behalf of the Jordan Department of Antiquities during 1956, apart from official duties, I conducted a survey over the area for evidence of Stone Age habitation. A large amount of surface material was collected, and in addition, and with the help of the B'dul tribe, two habitation sites were found (fig. 1). One was a neolithic village site, where a Natufian element was also present. This was situated in Beida. The other site forms the subject of this paper, and consisted of an Upper Palæolithic rock shelter in Wadi Madamagh.

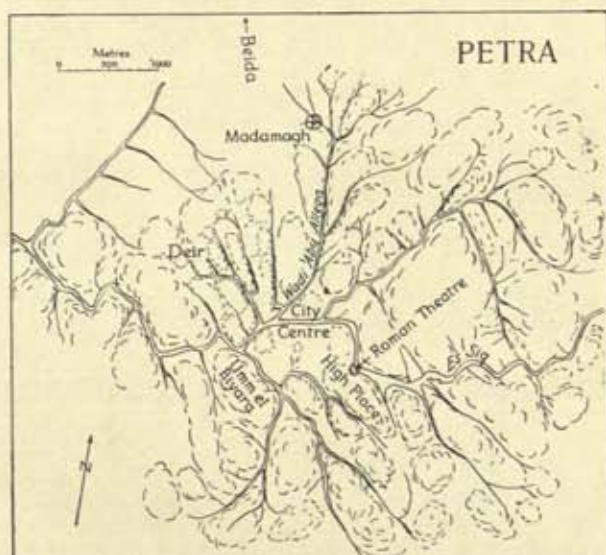


FIG. 1. SKETCH MAP OF THE PETRA AREA

After Canaan

This rock shelter is particularly interesting in that known Upper Palæolithic sites are rare in Transjordan. Dr. Waechter (1938) excavated one Kebaran site in Wadi Dhobai to the north of the country; the other Upper Palæolithic sites are recorded from the south (Zeuner, 1957). It is also important on typological grounds, belonging as it does to the final phase of the Upper Palæolithic, and consisting of an industry that is at present rather thinly represented in Palestine, Jordan, the Lebanon and Syria. The type site is at Mugharet el Kebara in Palestine. The predominantly characteristic implements of the Petra shelter and the other four are small backed blades with

obliquely truncated points at each end. At Petra, however, these backed pointed blades are extremely small. A normal microlithic Kebaran blade is large by comparison (fig. 2, 8).

The shelter in Wadi Madamagh (grid reference approx. 194/975 on the 1:250,000 map, Sheet 3) lies on the west side of a short and very steep *seyl* (seasonal torrent bed) which runs into the *wadi*. Wadi Madamagh lies on the northern boundary of Petra and was first discovered and named by Dr. Tewfik Canaan (1929). From the shelter one looks across the intervening Petra cliffs to Marmar el Burqa about a kilometre away across two very steep major *wadi* (Plate Ed). Marmar el Burqa, so named by the B'dul tribe, the present inhabitants of Petra, is the continuation of Umm Seihun, the open country of white sandstone which runs up to the foot of Mount Seir. The shelter is reached from the Petra valley basin by following Wadi Abu Alleqa to the end, and climbing across the high ridges to the west, finally descending into Wadi Madamagh (fig. 1). The site is most beautifully situated, and completely protected both from the weather and from wild animals. Set on a narrow ledge about 70 feet above the main *wadi* bed, the steep *seyl* by which it is sited falls precipitously in front of it, forming a vertical drop at the end of the talus and dividing it from a sheer face of rock on the opposite side which is the same height as the shelter, thereby affording complete protection with a clear view to the open country (Plate Eb). To the north, a vertical drop of a man's height leads to a steep slope down to the main *wadi* bed. To the south a short steep scramble brings one to a small open plateau entirely enclosed by vertical sandstone cliffs. The shelter itself is about 30 metres long by about three metres deep. Here, with their backs to the prevailing winds, but their faces to the early sun, protected from wild animals and at the same time being within view and easy reach of the open plains, palæolithic man might well have felt safer than in the deeper, wilder ranges which surround the valley of Petra proper.

The shelter has been partially destroyed or weathered away, as the deposit only begins about halfway along the roof overhang. The back wall has a thin deposit of flint artifacts and bones cemented to it. This cemented deposit begins about two metres nearer the south end of the shelter than the occupational deposit, which itself begins abruptly with the ragged side of a large hearth from which protruded numerous bones and flints. The impression gained is that the deposit had been cut away a considerable time ago as the cementation at the back of the shelter is so hard that it was impossible to remove a single flint from it, and the ragged edge of the deposit is also extremely hard. The present inhabitants of Petra would

\* With Plate E and two text figures



not have the necessary knowledge to use the deposit as a fertilizer for their fields, so it is suggested that the Nabataeans, who assiduously cultivated every square inch and *seyl* terrace, were responsible for it. The *seyl* bed is now dry, and at the mouth of the little plateau above the shelter is a rough wall built not as a dam, but as a terrace wall to retain the earth behind it. Thus the plateau formed a small field. It seems highly probable that the deposit was moved and placed on this field, especially as its missing part is that nearest the field.

Probably the terrace on which the shelter is situated originally extended forward for about another metre; at present the front of the deposit descends vertically on to the talus along its whole length as if cut off. This may be accounted for by the small track worn by generations of goats which runs along the entire front, or it may be caused by weathering due to the fall of some of the rock overhang. The talus must certainly have weathered considerably as it now resembles a very steep glissade down to a vertical drop. A number of very large sandstone blocks lying in the steep *seyl* bed argue for a large fall from the rock overhang.

Two trenches were put down, each yielding a main hearth area, some smaller hearths, innumerable animal bones, all in fragments as if they had been used for food and broken for the marrow, many flint artifacts and much waste material. Although there has been no opportunity to sort all the material the impression gained was that the deposit was homogeneous, the same microlithic blades appearing at the bottom as at the top. It seems possible that the shelter was used by one family unit over a number of winters. Details of the excavation and of the sections of the two trenches will not be given here. It is hoped that a full report can be published at a later date.

The microlithic elements associated with Trench A (1) were fully sorted and sketches were made in the field. Although the larger material from some of the levels was also sorted, it was not studied through lack of time. This paper, therefore, concentrates on the microlithic material, but the reader is asked to bear in mind the fact that a larger element exists. This cannot be too strongly stressed as it is not wished to give a one-sided impression of the industry.

*Description of the microlithic material.* Most of the artifacts are made of grey or brown flint, but some are of a very fine chert which occasionally shows silica glaze on the surface, probably due to lying in a hot fire. All the raw material would have to be brought from the limestone country outside, as flint is not obtainable in Petra.

1. Microlithic Bladelets. These bladelets are characteristic of this culture. They are long, very narrow and obliquely pointed at both ends. The backs are very steeply retouched, and there is not a single example of the Helwan retouch so typical of the Lower Natufian. The bladelets have been divided into ten classes based on very small differences, such as whether the backing extends down the points or not. It should, however, be stressed that the different shapes of point-backs running through oblique to rounded and near-lunates all shade into each other.

These bladelets outnumber all other artifacts in Trench A,

Level 1. They were presumably used as missile points or barbs. They were probably also used for working wood and perhaps bone. One dolomite pebble was found that had been carved, really a series of deep scratches, in lines running both round and over it, thus forming a pattern of small squares. The entire deposit was full of bones, all broken, and unfortunately bearing a hard incrustation. When these are made available for study, it is possible that evidence for working will be found on some. A number of marine shells were found, some pierced for suspension.



FIG. 2. ARTIFACTS FROM THE SHELTER OF MADAMAGH

Number 8 is from Level C at Kebara. Scale  $\frac{1}{2}$

There were also a large number of hæmatite lumps, some carved into sticks of pencil-like shape. Hæmatite is found in the bed of Wadi Madamagh itself, and elsewhere in and around Petra.

1a. Microlithic backed bladelets with points not retouched, or only partially so. Some of these are obliquely truncated. 70. (Fig. 2, 1a, b, c.)

1b. Microlithic backed bladelets with both points retouched along backs. 95. (Fig. 2, 2a, b, c.)

1c. Microlithic backed bladelets, broken. 30.

1d. Microlithic backed bladelets with both edges of one point retouched. 16. (Fig. 2, 3a, b.)

1e. Microlithic backed bladelets, obliquely snapped. Waste made from the manufacture of the bladelets, leaving these small backed blades with points on opposing sides. 9. (Fig. 2, 4a, b.)

1f. Microlithic backed bladelets with exceptionally fine nibbling retouch over whole or part of back. 24. (Fig. 2, 5a, b.)



1g. Microlithic lunate-type bladelets. These shade into their points in curves and not at an angle as in the other classes. They were probably made by the microburin technique. (Fig. 2, 6a, b.)

1h. 'Anti-microburins.' This term is suggested by Professor Zeuner for terminal portions of blades made in the manner of microburins, but snapped off so as to form an acute point. The snapping scar therefore continues more or less the direction of the remaining portion of the notch. This is a normal feature with geometrics made from the middle of blades, but specimens on the ends of blades are present at Petra. They thus appear to represent a distinct type. 3. (Fig. 2, 7a, b, c.)

1i. Microlithic backed bladelet retouched for half its length on opposing sides and ends. 1. (Fig. 2, 9.)

2. Notched blades. There are four small blades with deep notches worked in them. They all show signs of wear, and seem to be the ordinary spokeshaves and not unfinished artifacts made by the microburin technique. The fifth is one of the micro-bladelets, blunted along the back, but with a notch on the opposite side. It may have been intended as an artifact made by the microburin technique. 5. (Fig. 2, 10a, b.)

3. Hollow scraper. On the side of a small blade, there is a deep and long hollow, very like an elongated notched blade. 1. (Fig. 2, 11.)

4. Asymmetrical lunates. These are much larger than the other microlithic elements in this industry, being closer to the normal size of lunates. Both are asymmetrical, one comes very close to being triangular. The backing is by means of simple, and not Helwan, retouch. 2. (Fig. 2, 12a, b.)

5. Microburins. Three of these are waste from the manufacture of the larger lunates, and the others are from the tiny bladelets. 9. (Fig. 2, 13a, b, c.)

6. 'Hooks.' These are formed by making a notch just under one point of a bladelet. Three are on waste blades. 5. (Fig. 2, 14a, b.)

7. Asymmetrical triangles. Ordinary geometric microliths of which one might be called the normal size, and the rest are smaller. All are very long. 4. (Fig. 2, 15a, b.)

8. 'Spiky' points. On blades of normal size. Three have steep retouch, backing, up the whole of their lengths. Two are retouched from the point to halfway down the back but on the left side. Two are microlithic. 8. (Fig. 2, 16a, b, c.)

9. Missile points. Although the minute pointed bladelets were undoubtedly used as points and barbs of arrows this name has been given to a distinctive class resembling the 'spiky' points. In every case the points have been finely retouched on both sides. 13, of which two are broken. (Fig. 2, 17a, b, c.)

10. Obliquely truncated points. Some, but not all, are backed. 4. (Fig. 2, 18a, b, c.)

11. 'Spiky' missile point. This artifact has been placed in a class of its own. It is a point with delicate retouch right round its perimeter including the base. 1. (Fig. 2, 19.)

12. Points. Made on small waste blades with one end retouched on the back to form a point. 2. (Fig. 2, 20a, b.)

13. Elongated backed blades. These are in all essentials similar to the tiny microlithic bladelets except for size. They are bigger and longer, nearer to the true Kebaran. 4. (Fig. 2, 26a, b.) Compare with fig. 2, 8, which is a Kebaran blade from Level C at Kebara.

14. Crested guide flakes. These vary in size, but none seems more than 3 cm. long. 8. (Fig. 2, 23a, b.)

15. Broad blades and points partially retouched. 4. (Fig. 2, 24a, b.)

16. Backed blades approaching in size the ordinary Kebaran. 13. (Fig. 2, 25a, b.)

17. Irregular blades with backing. 18. (Fig. 2, 22a, b.)

18. Larger implements. Some of the larger elements were drawn, and are illustrated here to give some idea of their size and shape. (Fig. 2, 21, 27-30.) These comprise a thumb-nail scraper, a backed and pointed blade, a round scraper, an end scraper and a core scraper.

As has already been stated the most characteristic implements are the tiny backed bladelets with points at both ends. These are essentially the same as those found at Mugharet el Kebara in Palestine (Turville-Petre, 1932), and at Jabrud in Syria (Rust, 1950). Dr. Waechter, who has seen the unpublished material from Ksâr 'Akil in the Lebanon, tells me that these small pointed backed blades are also present in the top levels there. In fact, the difference in the Petra bladelets is their small size and extreme narrowness.

It seems that the ordinary Kebaran blades were made by striking an ordinary small blade from a core, and then retouching the back until the blade had been worn away for half, or slightly more, of its width. With the Petra Micro-Kebaran this technique is carried a stage further, i.e. to beyond the midrib, until only a sliver 2 millimetres wide is left. The ends are then shaped either by twisting off, or by retouching, and even by the microburin technique of notching and then snapping. Waste material bears evidence for the first and last techniques. The fact that the microburins, the 'anti-microburins' and the obliquely snapped waste—though doubtless they were used—were all backed as well is evidence for the extreme narrowness of the bladelets having been obtained by careful backing.

In comparing the Petra material with that illustrated by Turville-Petre (1932, fig. H, p. 275), one finds that the small pointed bladelets (nos. 17-25) are paralleled at the Petra site. Triangles (no. 16) are also present, in addition to round scrapers, end scrapers and core scrapers—although the latter implements from the Petra site can only be illustrated here by a few that were chosen at random.

Rust (1950, Plates CI, CII, pp. 107, 137), excavating Cave 3 at Jabrud in Syria, uncovered a culture in Levels 4, 6 and 7 which he called Nebekien and placed in the Middle Mesolithic. This Nebekien was divided in Level 5 by an industry that he called provisionally 'Spätcapsien (?)'. The characteristic implements of Nebekien Levels 6 and 7 (Plates CI, CII) are again the small backed bladelets with obliquely truncated or curved points. These show a close relationship with those from Kebara Level C, and also with Petra. Jabrud, however, produced a number of unmistakable microburins, and these have apparently not yet been found at Kebara. Petra, which also possesses microburins, has a further link with Jabrud here.

One class of implements strongly represented in Petra, the 'spiky' points, is lacking in the illustrations at both Kebara and Jabrud. On the other hand, they seem to be one of the characteristic implements found in the upper levels at Ksâr 'Akil in the Lebanon. This site has not yet been published, but in the preliminary report (Ewing, 1947), this culture is called 'Gravettian,' and was found above the Aurignacian. However, as the small backed pointed blades of the Kebaran are also present in the 'Gravettian' at Ksâr 'Akil a further link between Ksâr 'Akil and Petra seems to have been established. Until the material from Ksâr 'Akil has been published and the material from Petra studied in detail it is impossible to be more definite.

The fact that the Petra site appears to be homogeneous,



the bladelets appearing from the top to the bottom of the deposit, makes it impossible to assign the industry to its place by the section and sequence of cultures. Typological evidence alone must be relied upon. Comparison of material shows that the closest links are between Kebara Level C, the type site, Jabrud Nebekien, Ksâr 'Akil 'Gravettian,' and Site K at Dhobai in Transjordan. There are also connexions with the backed and pointed blades of el Khiam, from the Upper Palæolithic VI level (Neuville, 1951, fig. 66), reminiscent of the tiny bladelets of Petra, though of course much larger. I am, therefore, calling the Petra industry Micro-Kebaran, and placing it in an intermediate period between the end of the Upper Palæolithic and the Natufian. When available, the material obtained for a Carbon-14 test should help to assign the Micro-Kebaran to its proper place more securely.

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## BRONZE AGE TECHNOLOGY IN WESTERN ASIA AND NORTHERN EUROPE: PART III\*

by

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## THE BRONZE AXEHEAD

64 There is enough similarity in the axehead industries of Asia and Northern Europe to suggest a common origin, and enough difference to confirm an early independence of the barbaric European mind. One might almost say that the conquest of Rome by barbarians may be seen to have begun with the independent development of this all-purpose tool and weapon.



FIG. 1. SHAFT-HOLE AXEHEAD FROM UR IN THE BRITISH MUSEUM

The metal guard covering the blade edge is clearly visible. Length: 7½ inches

At first, in Egypt, Asia and Northern Europe, the bronze or copper axehead shared a common basic flat form derived from the stone celt. The civilized Asiatics quickly solved the mechanical union of metal to wood, in pre-fabricating a clay core which, inserted into the mould, formed a hole through the axehead, as with certain stone axeheads, or with the modern hammer, to receive the

\* With eight text figures; reference should also be made to the lower picture in Plate C in the February issue of MAN. Part I appeared in the February issue (1958, 13) and Part II in the March issue (1958, 39). Cf. also Professor Thompson's article in MAN, 1958, 1.

shaft of the wooden handle. And that was that problem settled once and for all, in early Sumerian times, as may be seen in the example from Ur (fig. 1; this example, it is interesting to note, bears evidence of its owner's concern for the safe keeping of his carefully whetted edge in the provision of a metal shield).

In Northern Europe barbarian smiths were occupied with this one problem of the mechanical union right up to the end of the bronze age. The mechanical union passed through endless development and was still being rigorously experimented with in the final socketed type. Previous to this final type, development had been through a succession of other well-known types: the flat; the flanged; the stop-and-flange; the (plain) palstave and the winged, before the final socketed. It is a rare thing to find examples exactly alike in any of these types.

Of the ultimate solution in the socketed type, it has been said that it is mechanically inferior to the Sumerian. Let it be granted, and that the barbaric smiths learnt no more than the limitations of many alternatives. Such vast negative experience certainly kept the barbaric mind supple and receptive to improvement throughout all, for even at the last their restless desire for improvement continued in the variety of form which they gave to the interior of the socket by varying the pre-fabricated core. The socket interior might be classified in its progressive variation by its section as: round; oval; square and rectangular. Each of these has many minor modifications in the ceaseless measures taken to restrain the axehead from working loose on its haft, by twisting as the blow is delivered.



The idea of dividing the stock of the handle must be given to the Stone Age (boring a hole in a straight haft). The flat metal celt took over the same idea and divided the short leg of the angle haft—the same mechanical principle, improved somewhat for the somewhat different form of metal with its greater tendency to split the haft. This division of the short leg of the haft continued to serve until the arrival of the socket.

The flat celt grew first flanges which were turned to additional utility in harbouring the divided haft and preventing celt waggle in one plane; it then acquired a bottom stop to restrain the shank from driving back between the divided haft, and now needed only a little more time to drop the continuation of the flanges beyond this stop and so become a true palstave. A palstave it now was with a walled slot on each side of its shank, into which the ends of the divided haft were bound. The walls of the palstave slots no longer grew beyond the stop but yet continued to grow into wings; that is, in height sufficient to be hammered over the haft ends as an improvement on binding them in. All these early forms made their contribution to preventing the celt from loosening by torque, and the eventual abandonment of the divided haft on account of its own mechanical weakness.

The palstave, then, was preceded by the stop-and-flange form and this by the flange without stop, which was the first improvement on the flat celt. Coming forward again over these successive types, we find that kinetic image, the outcome of many 'stills' as in cinematography, of the first flat celts hammered out of natural nuggets of copper as the pupa from which the winged and then the socketed forms emerge. Natural copper nuggets—melted copper—cast copper (or poor natural bronze)—these precede the artificial true fluid bronze, the greatest secret of the gods snatched by promethean smiths for the technological benefit of man. For the most intricate forms could be deftly modelled in soft beeswax and miraculously translated into hard bronze. Hitherto, puddles of poor bronze, smooth on the lower side but tough and scabbed on the upper, had to be wrought smooth on both faces by hammering, which also hardened the shank and tempered the blade. The shank, thinning under repeated hammering, had to be packed thick by edgewise blows, and this raised an edge burr which was deliberately increased to form flanges intended to restrain the wobble of the celt in the vertical plane. The extent to which these flanges might be produced was limited by the laborious nature of the work. With the advent of the wax process the flanges were easily drawn out by the fingers; and then what was simpler than to make a two-piece clay mould from a bronze model, to fire it for durability and to reproduce the wax models in quantity thereby? At this point in the story, the concomitants of such progress included an increased demand for such serviceable celts which was only to be met by further improving the reproductive mould, *viz.* by casting it in bronze.

Good waxes were obtained from smooth-faced two-piece pottery moulds. The shortcoming of pottery as a material for wax-reproduction-moulding was its friability,

chipping at the projecting arrises and rubbing away in use at the keying devices—dowels (better, natches), or beading and groove. For quantity production, they needed frequent renewal by copy moulding. Compared with bronze as a mould material, they were not ideal, for pottery being porous had to be soaked in water to keep the wax from entering in. If this should happen, then the mould had to be re-burnt.

It seems that a little casting of metal in stone bivalve moulds must have taken place before the general advantage of wax-reproduction in pottery moulds became established; for the few celts usually thought to be malformed on account of the two halves of a stone mould slipping out of correspondence after being bound together might not occur with waxes reproduced in pottery moulds, no matter how badly worn, since a faulty wax would be spotted and either rectified by retouching the wax or discarded as beyond remedy.

The bronze reproductive mould for waxes, when it came, must have seemed at the time to possess the prospect of the epoch-making importance which is due to it in retrospect, as we may see. The bronze mould is more efficient: absorbing no wax; having a smoother surface and better conductivity than pottery to chill the wax more effectively; and readily taking close-fitting, escapeless, labyrinthine bead-and-groove closures of the two halves, which for the purpose of wax-reproduction were everlasting. With bronze moulds perfect waxes might be easily produced which needed no retouching. Bronze moulds continued to be the last word in celt industry until the restless concern for ever improving the union of metal and wood found it necessary to give designed form to the socket interior. To form the socket with a pre-fabricated, rather than a natural, core was a project rather beyond the power of the two-piece bronze mould alone.

In consequence, the pottery mould, already superseded for wax-reproduction, was now recalled for the reproduction of a pre-determined core form.

*Core forms.* Here are, briefly, some of the impressions which I had either to confirm or to reject when, in making casts of celts in bronze from wax models taken from two celt moulds in the British Museum (fig. 2f and Plate Ck, l), I realized that the first-born socketed celt cast could have had a natural core (fig. 2g). To re-enact the casting of this first natural socket form, I used one of the plaster moulds (Plate Co, p) and filled it with wax. After allowing this wax to thicken on the inner walls, I emptied it out, leaving a 'natural' internal cavity which when filled with clay would become a natural core united to the clay of the mould. I then took a flexible cast of this internal natural cavity and found that it echoed the waisted form of the celt (fig. 2g). The next thing was to take a flexible cast of the (pre-fabricated) core from the original celt from the Quantock mould (fig. 2f) for comparison with the natural core.<sup>1</sup> I then looked for celts with natural core forms, and found several in various collections. But, judging from the small proportion of them to others with designed cores, the natural core must soon have been abandoned, in spite of its great advantages in being more easily made,





FIG. 2. SOME DETAILS OF BRONZE-AXEHEAD-PRODUCTION

(a) Modern jeweller's crucible (capacity 1½ lb. bronze; thickness of walls less than ¼ inch); (b) normal destructible one-piece clay mould (burnt out) containing fireclay core inside the cavity previously occupied by the wax model as in Plate Cp; (c) half the original palstave mould in bronze in the British Museum (also reproduced as (a) in fig. 5); (d) original pouring cup in the British Museum, formed by three plugs or wedges, as its seams show (see fig. 3); (e) a bronze tamper head (?), with the remains of its wooden handle, designed to 'tamp up' the clay filling the core mould (as Plate Cn).—N.B. Although a stick might tamp up the clay in the centre of this mould, it would not compress that by the walls as this cusped head of bronze would do; any variation in core density increases the risk of fracture by shrinkage during the burning-out of the wax—; (f) half of the original Quantock mould in the British Museum with a pre-fabricated core correctly located inside it by the core trunnions; this core is from the core mould in Plate Cn; (g) a flexible cast taken from the bronze cell, Plate Cm, which was cast from a wax, Plate Cp, taken from a plaster cast of the original mould, Plate Ck, 1. Length of (c), 6½ inches; of (f), 5 inches.

and stronger, by reason of being in union with the clay mould.

*Disadvantages of the natural core.* One great advantage which it had was offset by two troublesome disadvantages. The waisted form (fig. 2g) made it very liable to break off either during its first shrinkage by air drying or in its second shrinkage during partial sintering of the core when the wax was being burnt out of the mould. Moreover, the waisted interior permitted the tip of the haft inside it to develop wobble. In effect, celts could not be given a flare in the blade because this would result in a socket with a choke or waist in it. In consequence, the core had to be designed.

*The designed core.* The designed core tapered more towards the blade end (Plate Cn, o and fig. 2f) and was clearly reproduced in a core mould of two halves. The core moulds were of pottery. (I made mine in plaster from the

core cast in gelatine. Plaster was softer, but hard enough for my purpose and it had the same absorbent property as pottery, so necessary for moulding cores in clay—to take up the water in the clay compound.) For this reason, no bronze core moulds have come to light, for all core moulds were made of soft-fired pottery and do not survive, for they have broken and perished. The core was suspended from its trunnions or shoulders, which engaged with similar forms on the mould and located it centrally within. Core forms exist in a great variety of sections, with and without grooves which gave ribs to the socket interior calculated to improve the union of bronze head and wooden haft. Other features of the core will be mentioned as we come to them.

*The bronze mould in operation.* Smooth and almost impervious bronze was admirably suited to casting wax models. With a smear of soap I prevented the wax from



sticking to it. First, the core was placed with its trunnions engaged with the lugs in one half of the mould (Plate Co), which was then closed with the other half and bound round. Two gaps were left open by grooves half in the neck of the mould and half correspondingly on the head of the core. These gaps were now filled by two wedges, inserted into them to steady the core and to take a saddle-shaped piece in between them (fig. 3). A band of clay was wrapped close around the top of the mould and then the pegs and saddle piece were withdrawn, leaving a space which when occupied by wax formed the pouring cup and its jets (fig. 2d and Plate Cp). When the wax had been poured and had solidified, the mould was opened and the wax model taken out with the clay core centrally embedded in it (Plate Cp). The whole operation took only a few minutes. The seams on the wax were trimmed off with a knife, and any imperfections might have been retouched—but there were none. Evidence for the use of pegs and saddle piece was detected in the seams which they made on a sprue cup in the Stoke Hoo hoard in the British Museum (discernible on fig. 2d).



FIG. 3. DIAGRAM OF THE THREE WEDGES

*These are inserted between the bronze mould of the socketed celt and the head of the pre-fabricated core and are removed before pouring the wax; evidence for this is found in the seams on sprues (cf. fig. 2d).*

**Pouring the bronze.** The wax model (Plate Cp) with its core in place was now invested (fig. 2b), that is covered with grog (pre-fired and pulverized potter's clay) bound by an admixture of plaster in the proportion of one part to four. It was then wound with a resistance wire of nickel-chromium alloy and the current turned on. It was held upside down while the heat first melted and caused to run out whatever wax was not soaked up by the porous mould fabric. Now it was ready to be embedded in an insulating material to conserve heat and burn out the soaked-up wax completely at 800° to 900° C. The bronze (1½ lb.) was then put into the crucible (fig. 2a) which was set in a furnace, alongside of which was stood the red-hot mould, and, when melted, the metal was poured. On cooling, the mould was broken open and the celt released (Plate Cm). This small amount of metal was melted in order to simulate stringent economy. But where the conditions probably exceeded those of the Celtic smiths was in my use of a thin jeweller's crucible (fig. 2a), as will be seen presently. Actually the casts of Plate Cm took one pound of bronze and those from fig. 2f only nine ounces; this was calculated beforehand by weighing the waxes without cores in them.

**Finishing.** Little was needed to complete the celts for the reception of their angle hafts. The jets had been tapered or 'grated' where they entered the collar of the celt, to facilitate removal, presumably with a bronze chisel. (I used a

fine saw.) I then hardened the blade of one celt by hammering it on an anvil (cf. fig. 4; my own anvil was a home-made steel one), which drew it out a little. Sharpening was then done by whetting the blade edge on a grit stone (the celt in fig. 4 has been so whetted). Several hoards contain slightly domed bronze anvils like that illustrated, for hammer-hardening the blade edge.



FIG. 4. HAMMER-HARDENING THE BLADE EDGE

*A bronze anvil for hammer-hardening of the blade edge of the socketed celt (Plate Cm) with a socketed bronze hammer head on the left. (The straight side of this anvil is due to removal by chiselling of a faulty area of spongy metal.) Greatest diameter of anvil, 6½ inches.*

**Difficulties.** The two difficulties needing most care and attention during the whole casting process were: (1) the moulding and handling of the core and (2) the manipulation of so small a melt of bronze in removing it from the furnace and pouring it in time, before it had lost too much heat to run well in the mould. I have no doubt, in fact there is evidence, that the celt smiths used much thicker crucibles, which would reduce the rate of heat loss, and evidence too that they used a vitreous flux of sand to cover the metal in the crucible and prevent the formation of surface dross by oxidization. Many such crucibles coated on the rims with vitreous flux were found in a smith's shop site in Sweden and one is reproduced in *Göteborgs och Bohusläns Fornminnesförenings Tidskrift*, 1940, p. 101. On p. 103 of the same are reproduced several small fragments of pottery moulds used for casting wax models of ornaments, the demand for which never reached mass production necessitating the use of the bronze mould.

**Core shrinkage.** The celt smith must of course have been aware that shrinkage took place in the air drying and then in the 'burning-out' of his moulds. But whatever measures he took to deal with it in the earlier palstave and winged-celt castings without cores, he certainly found that when a core was employed the shrinkage of his investment needed more understanding. The core being suspended within the mould was liable to break off and the broken



piece floating in the molten bronze would surface and spoil the cast, as I frequently found; there are many examples of this in the scrap metal of various hoards. This weakness must have been greatly exaggerated in the waisted core, for its waist made it subject to two centres of contraction, in the larger bodies each side of it. The suspended core had to be strong enough to withstand the pressure of a rush of metal down one side of it. The curious tool of 'unknown use' (fig. 2e) I take to be a tamper or rammer for the purpose of packing the core clay as tightly as possible in the core mould, to minimize the first shrinkage in drying the core and the second in burning out the mould at a high temperature. A good taper was found to reduce the risk of fracture as the shrinkage then proceeded in one direction only, from the tip to the base held in the body of the final clay mould by the same trunnions which had held it in the bronze one.

*Mould-reproduction.* The evidence may now be cited upon which is based the statement made at the beginning (Part I) that many a palstave or socketed celt may be much earlier than the bronze mould which it is found to fit into. Many slight variations in design of the master moulds were no doubt considered to have some advantage, as they still do in models preferred by both war lord and merchant. Old forms die hard.

In the British Museum, there is one half of a palstave mould, with the impression of two bands of binding twine cast in bronze on its external face (fig. 2c and fig. 5).

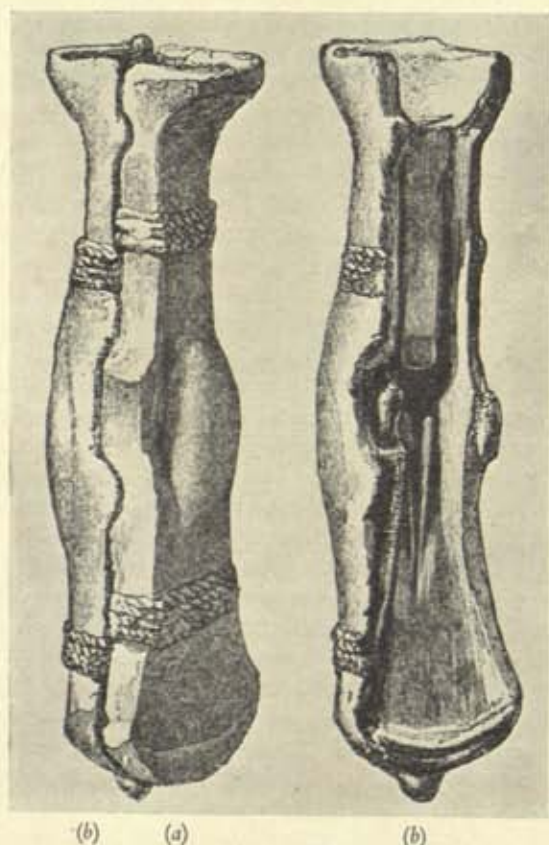


FIG. 5. EVANS'S ENGRAVING OF THE BRONZE PALSTAVE MOULD (FIG. 2c)

Evans includes a wood engraving (fig. 5) of this mould, complete with its other half, in his *Ancient Bronze Implements* (p. 440). This other half cannot now be traced in the British Museum for closer examination. But even without it, one can see from Evans's illustration that the twine bindings do not correspond on the two halves. Without the insight to be obtained only from moulding experience, Evans incorrectly assumes that this copy mould was moulded on an actual palstave, and he tentatively proposes a precaution against accident by the moulder with the pieces as he finishes them as a reason for the oddness of the binding bands.

We may well ask why a moulder making a two-piece mould from a palstave should bind the portions on to the palstave as he completes them, instead of taking less risk of breaking the dried pieces of clay by placing them safely aside. If he had in fact been making the mould from a palstave, it would most certainly not have old, decrepit, worn-out joint-locating devices in its 'joggles' and fragmentary remains of a bead and groove. Then again, even if the moulder had available both the palstave and the pottery mould from which its wax was taken, he would surely choose to copy-mould from the pottery mould and not from the palstave. For if he used the palstave, it would mean six mouldings—two positives in clay and four negatives from these as flasks in which to pour the bronze; and certainly the jointing devices would be fresh and new. Whereas, if he used the old and worn-out pottery mould, only four mouldings, to produce in clay two flasks into which to pour the bronze (fig. 6), would he need to make from them; and of course the old and crumbling pottery joint devices would then be reproduced in bronze—as in fact they are.



FIG. 6. ORDER OF COPY-MOULDING FOR BRONZE MOULD (FIG. 5)

This illustrates the reconstructed order of copy-moulding from the original pottery mould (not extant) from which the bronze palstave mould (fig. 2c and fig. 5) was taken; I and II from the mislaid half (fig. 5a), III and IV from the half shown in fig. 2c and fig. 5b.

Even without the mislaid half (fig. 5a) of the bronze mould, it can be confidently stated that this was the first half to be moulded. As Evans's wood engraving shows, the bindings on the first half occupied the optimum position at the narrowest parts, top and bottom, and therefore the bindings on the remaining half (fig. 2c and fig. 5b) are on the tapering surface alongside of them, being made in the second place to avoid overlapping the bindings on the first half.

That waxes were in fact reproduced in pottery moulds, before the bronze moulds replaced them, is corroborated by this unique specimen. Unless this is a bronze copy of an old and worn-out pottery mould of a favoured pattern,



then how are we to explain that none of the working parts in the closure of this mould have the character of these parts in bronze moulds? These are ill-fitting, oversized and worn 'joggles' and the fragmentary remains of a coarse and worn pottery bead-and-groove joint. These jointing devices had to be large and gross in pottery moulds, on account of this material's friable nature; they were subject to wear by friction both in and out of use, in the smith's tool kit. Small and precise closing devices in dowels, slots and rabbets or beadings were not only possible but invariably made in the more durable bronze moulds.

May we not then fairly deduce from this one available half-mould, and Evans's illustration of the other, that the moulder lived at, or about, the time of the introduction of bronze moulds for wax-model-production and, wanting to preserve this old and worn pottery mould, now on its last legs of service, cast it in the new and stouter medium of bronze in order to get more service from it? Why are its jointing features clumsy 'joggles' and defective bead and groove, which at least struck Evans as singular?

The sides are joggled together in a singular manner. As to the bands of cording, it may be that the first half of the mould was formed of clay, which when dry, in order to prevent its being broken, was tied on to the palstave on which it had been shaped, and thus moulded in clay or loam; and that afterwards, when the second half of the mould had to be cast by a similar process, the mould for it was tied on to the half already formed, the binding being in contact with the side of the band already in relief on the back and sides of the half-mould.

The procedure, in my view of this being a copy mould from an old pottery mould for the production of wax models, may now be stated. For simplification of the visual image of what took place, let us suppose that the moulder is moulding the two halves of an Easter egg, the jointing rabbet of which is old and crumbling, but that the halves were otherwise in better condition. If it be assumed that the moulder first takes the moulded impression in clay of the external or convex face of one half of the egg, it will be plain sailing to explain the function of the bands of cording.

Now it will be seen to be well-nigh impossible to hold one half of an Easter egg in the hand while with the other hand moulding its convex side with a loamy clay. Yet, if the two halves are bound together, top and bottom, the one half may be held while the other is being moulded. (As the bronze impression of the twine bands of the palstave mould shows, something like wax or a smear of clay was used to fill in interstices of the twine and so prevent any undercut on it from tearing the surface of this half-mould as it was lifted off.) The twine must be cut and the unmoulded half-egg removed to mould the inside or concave face of the first half, including the broken rabbet joint of the egg and, beyond it and flush with it, the face of clay thickness to the convex half-mould, with depressions made in it to serve as location keys like those in the core mould (Plate Cn). Both inner and outer faces of the first half-egg completed, they are parted and the half-egg taken out, to be bound again to the unmoulded half to repeat the process. The second binding avoids overlapping the twine on the first half for obvious reason, and is placed close to

it as seen in Evans's illustration of the complete Wiltshire palstave mould. To a moulder of wide experience it should be evident from what has been said that this copy mould was taken from another mould and not from a palstave as Evans assumes, and that therefore palstaves cast much earlier might be found to fit into it.

*Conclusion.* Nothing could be simpler and more direct than the original practice of the *cire perdue* process: modelling a form in wax; embedding it in clay; burning out the wax and pouring in the bronze. (It has been largely brought back into practice during and since the Second World War.) Metal form and finish could not be better achieved than by casting in a one-piece mould formed on a wax in a fine loamy clay. For unique works of non-practical form this simple sequence, wax—clay—bronze, still continues to serve.<sup>2</sup> But when the supply of tools and weapons became a specialized industry, as the demand for them increased, the simple translation of form from wax to bronze became increasingly more indirect and industrial. Repeated modelling of the wax form by hand was the first bottle-neck in production to be removed by industry through the introduction of innovations between the initial wax and the final bronze. These auxiliary processes became so permuted by the end of the bronze age and the time of the socketed celt that the enquiring mind—without some experience of all auxiliary means—became easily persuaded by the more obvious or symmetrical possibilities.

The evolutionary order of celt-production by bronze-casting, calculated upon a caster's experience of the line of least resistance—in reproducing the wax forms from one original and thereby overcoming the most laborious and exacting operation (*i.e.* the making of a wax form by hand-and-eye judgment for each celt cast) was most probably the following. (a) A hand-made wax model, invested in a one-piece or destructible mould, in which case each bronze cast was from a unique hand-modelled wax. (b) In breaking down this one-piece mould to release the cast, large pieces of it would come away intact; a fact leading to the reproduction of bivalve moulds, in two halves, taken in clay from a bronze celt. It is extremely doubtful whether these, in a soft-fired clay, could be made to serve more than once. But clearly their chief advantage was in avoiding a re-modelling of the original wax form. An inherent disadvantage in this method, however, was too formidable to be neglected for long, that is the difficulty of the finishing, in cleaning off the metal seams or seepage between the mould halves. It was soon seen that if the pottery (or stone) mould were used to cast waxes in, the seams could be taken off much more easily in the wax. (c) This stage has already been fairly well described in the advantage of the practically everlasting key devices of the two-piece mould when made in bronze over the earlier form of pottery moulds. As to whether the bivalve mould in stone (principally used for spearheads) precedes the pottery form there may be two answers: the experimental precedence and that of general practice. The stone moulds which I have seen would serve as well as pottery ones for casting waxes. For bronze-casting there is the same problem of cleaning off the seams if either stone or pottery



moulds were used in the earliest attempts at a reproductive method.

In the course of this study I was naturally led to examine early coinage by the suggestion contained in Sennacherib's reference to the half-shekel as a well-known small casting. I spent a pleasant fortnight in the Department of Coins and Medals at the British Museum and in related experiments in my studio. On account of the limited time at my disposal I was unable to continue the examination of coinage technology, and also because I have no experience of the behaviour of metal when die-struck. But from what little I saw of Greek primary, early Indian, Etruscan, Roman and Early British (Belgic) coins, I remain persuaded that coinage began with a casting technique. The Greek coins being the first were in my view not struck in metal—gold, electrum or silver—but were struck in another material and then cast. They have all the appearances of the behaviour of beeswax when struck by a die. I did go as far as producing a cast die (of bronze) and striking beeswax pellets with it. These were taken to the British Museum and compared with the coins under a strong glass. Though other studies in prospect prevent me from going further into coinage technique, I offer to others for what it is worth the suggestion that Greek primary coins were made by casting pellets of metal in beeswax calculated to the differential specific gravities of it and gold, electrum and silver to weigh their worth. These pellets were then struck with metal-cast dies bearing the device on one or on both faces. Jets of wax were then added to them (there are signs of their careful removal). And over-striking, to which beeswax is extremely liable, accounts for the rugged crystalline character of splits round the edges, in many of the larger of which appear cast forms of small bubbles of air in the fine-grain investment material in which the struck wax models were immersed. When and by what degrees the casting of dies for pressing out clay moulds or for striking waxes was resolved into the cutting of hard metal dies for striking softer metal blanks is a very interesting and crucial point which coinage may offer the best opportunity to study; and, incidentally, it is a transition followed by tool-makers much later on.

The first flat celts were hammered out of natural copper, and then out of rough casts or puddles of it or of poor bronze, smooth on the lower side, rough and scabbed on the open side. The rough or poured side, distorted by cooling shrinkage, had to be wrought smooth by hammering, which also served to render the shank hard and the blade tempered. Edgewise hammering, to pack back the thinning shank, burred it over at the edges and it was this edge burr that was increased until it became the flanges, once it was seen that they restrained the wobble of the celt in the plane of the stroke. The extent to which the flanges might be developed by hammering was limited by the laboriousness of the work. When the wax process came, the flanges were easily drawn out by fingers in the wax. A two-piece clay mould reproduced wax moulds of these raised flanges, and raised the industry from a stage of unique wax-model-production to a reproductive one.

By this time the concomitants of such progress included

an increased demand for such celts that was only to be met by further improvements in reproductive moulds. And these were made in bronze.

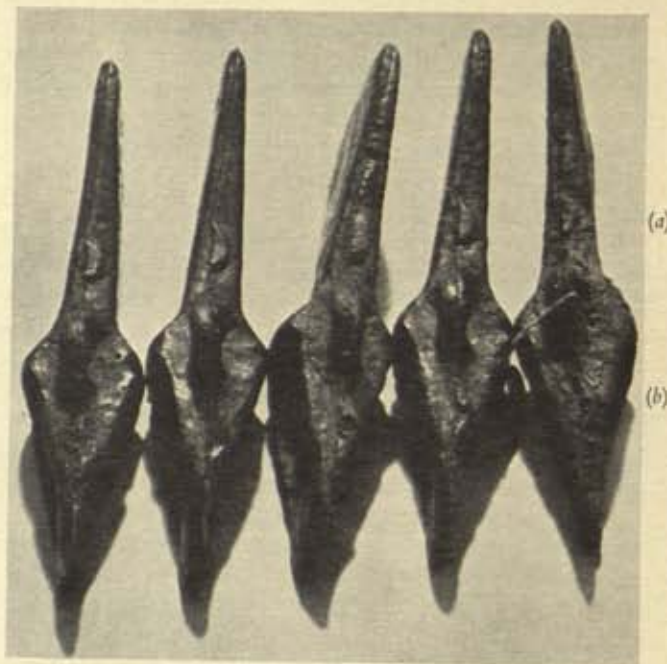


FIG. 7. PALSTAVES FROM THE STIBBARD HOARD

Five of 17 cast bronze palstaves from this hoard in the British Museum, all taken from waxes from the same mould (most probably of pottery). At levels (a) and (b) may be seen on each the stub ends of wax runner and riser, as trimmed off before investing the wax in loamy clay. Length, c.  $4\frac{1}{2}$  inches.

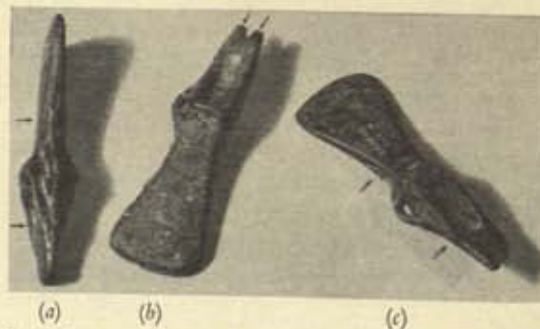


FIG. 8. SIDE VIEWS OF STIBBARD HOARD PALSTAVES

Three of these palstaves tilted variously to show both stubs of wax runners, indicated by the arrows at (a) and (c), and those of bronze runners, indicated by the arrows at (b). As is seen more clearly in fig. 7, the channels forming runners in wax have been made only in one half of the bivalve mould, probably scooped out of the closure wall before firing the clay mould. The two metal stubs are of varying length: those in (b) show the natural form (i.e. the curve of surface tension) of the head of metal poured; others are longer, probably including the cup, and have been broken off.

If (c) is viewed with the loop uppermost, it will be seen that the wax runners, beneath the downward-pointing arrows, are not at the highest point, since the loop rises above them. Wax poured into such a flask mould would therefore not reach and fill the loop. This indicates a measure taken to economize in the use of wax (beeswax being in short supply). The method in use was therefore to fill the mould with wax and then turn it upside down to run out. The result would be a thin hollow cast of wax with a solid loop. At a guess only about one-tenth to one-quarter of the wax in a solid cast would be needed to make such a hollow cast.



The evolution of practical form in the bronze age involves the same element of polemical controversy—as in accident or design?—as the origin of species. Both may be argued either way, and all that may be said of such arguments is that they are expressions of the dual constitution of the human mind.

I am prepared to make considered answer to any informed criticism of or observations upon my reconstructions.

#### Acknowledgments

I am indebted to Mr. R. D. Barnett, Keeper of Western Asiatic Antiquities in the British Museum, for helpful suggestions and to Mr. D. J. Wiseman of the same Department for help on the interpretation of the cuneiform script of Sennacherib's description; to Mr. J. W. Brailsford and Mr. G. de G. Sieveking of the Depart-

ment of British and Mediaeval Antiquities for many facilities; to Mr. John Walker, Keeper of Coins and Medals; to the technical staff of the British Museum, especially Mr. L. R. Langton, for valuable assistance; to Mr. H. H. Coghlan, Curator of the Borough Museum, Newbury, for his careful inspection and consideration of my casts and moulds; to my son John for the photographs; and to Mr. William Fagg, Deputy Keeper of Ethnography in the British Museum, for much help and encouragement without which I could not have proceeded.

#### Notes

<sup>1</sup> All casting was done in my studio with plaster copies of the celt moulds supplied to me by courtesy of the British Museum, except that the Quantock socket cast in gelatine and all waxes from the Scythian arrowhead mould were taken in the Museum.

<sup>2</sup> In art bronze foundries, where the usual thing is to take only one cast from the model in wax for a unique copy in bronze.

## SHORTER NOTES

**A Comment on Dr. Leach's 'Trobriand Medusa.'** By Ronald M. Berndt, M.A., Ph.D., Anthropology Centre, University of Western Australia

65

Since the publication of Dr. Edmund Leach's paper 'A Trobriand Medusa?' (MAN, 1954, 158), I have been intending to comment on his ingenious interpretation of a stylized design on a Trobriand Island shield. Three years have elapsed since then, and no one in that time has, to my knowledge, discussed his contention in the light of Malinowski's empirical material. This has surely not been because British anthropologists and others are unfamiliar with Malinowski's work (so recently re-evaluated)!

The method which Leach adopts to impute meaning to this Trobriand shield design is one which, given some empirical data about the relevant society, relies heavily on inference and intuition. I shall not consider here whether or not the methodology involved is anthropologically legitimate; I have discussed this aspect in another paper ('Some Methodological Considerations in the Study of Australian Aboriginal Art', to be published in *Oceania*). I would hold that it is not, even though the information which he draws on to 'explain' his interpretation has to do, directly or indirectly, with the relevant topic of warfare. Be this as it may, my comment concerns certain material supplied by Malinowski in a volume not referred to by Leach in his paper—that is *The Sexual Life of Savages in North-Western Melanesia*, 1929 (references in this note are, however, to the 1939 edition). Taking into account what is available here, I suggest that it is stretching the point too far to suppose that the stylized design (illustrated in Leach's fig. 1) on the Trobriand shield represents a flying witch. My disagreement with this interpretation rests primarily on the fact that Leach, in his analysis, does not take the cultural context sufficiently into account.

In the Trobriand Islands, if we are to rely on Malinowski's material (and there is nothing yet to the contrary, since Dr. H. A. Powell has not yet published his work on 'The Trobriands Revisited'), male sorcery, and presumably 'war magic,' is 'concrete,' 'almost a rational system' in contrast to female sorcery. There is apparently a great difference in the forms of sorcery performed by members of the two sexes: the 'character changes entirely with the sex of the practitioner. Man and woman have each their own sorcery...' (Malinowski, 1939, p. 38). Further (p. 39), we see that the 'flying witch' belongs solely to female sorcery—'she is always a real woman...'; and the difference between this and male sorcery is made clear. 'Witchcraft is inherited from mother to daughter...' Another 'most important difference between male and female sorcery lies in the

fact that the wizard actually carries on his trade, while the witch's activity exists only in folklore and in the imagination of the native...' (p. 40).

In other words, we would not expect to find any association with a flying witch in the context of warfare, an exclusively male activity. Therefore, we would not expect the design used on war shields to be representative of such a witch: not because of its being a female figure, but because the witch is the 'property' only of females. War magic (which Malinowski says was in abeyance during the time of his fieldwork) 'was an hereditary system of spells and rites always practised by a man of a certain sub-clan' (p. 36).

We are thus back where we started, since Leach's hypothesis concerning the design on a Trobriand war shield is evidently not relevant when viewed in its proper setting, and in relation to its social implications. Let me, however, erect another hypothesis which takes these into account.

A feature of warfare in those islands was the demand that men taking part in it should temporarily abstain from sexual intercourse, and from all contact with women (Malinowski, 1939, p. 383). Such a tabu, says Malinowski, expresses the idea that sex is incompatible with certain activities (in this case, war). The importance of this tabu is made explicit by him (on pp. 414f.), and offers a key to the interpretation of the design. Complementary to this is the Trobriand assumption that copulation is a fitting subject for verbal abuse; the strongest terms refer to sexual relations between the person addressed and his wife (pp. 409f.), which in the ordinary way are virtually 'unmentionable.' Abuse can deliberately be used to shame the person or persons so attacked (p. 408).

The conventionalized shield design is thus a representation of a male and female engaging, or about to engage, in coitus. The lower half of the design (at the bottom or wider end of the shield) is the male, with the penis quite prominent, and the upper half the female in an appropriate position. (The design conforms with the position most usually taken during the sexual act: see Malinowski, 1939, pp. 283f., 288f.). Shield designs vary within a certain range; and in the one in my possession the sexual act is perhaps more apparent than in the one illustrated by Leach, though even there it is unmistakable.

The shield is no representation of a petrifying witchdragon Medusa, having (as Leach suggests) the same logic behind it as we find in the story of Perseus, nor even a 'sort of Melanesian Rorschach test.' First, it serves as a visual form of abuse against the



enemy, designed to shame them, to show the bearer's contempt for them. Secondly, it contravenes the strict rule concerning the essential incompatibility of war and sex, and is therefore appropriate in this context and in this form as a mechanism of ridicule. Primarily, the shield design is a vehicle through which derision is expressed—to demoralize the enemy on one hand, and build up one's own confidence on the other.

Although this interpretation seems to me to be directly relevant, and to conform with what we know of the local situation, since we have direct empirical substantiation from Malinowski's own writing, we cannot be sure that it is the only possible one, or that Trobriand Islanders living during the time of Malinowski's fieldwork or before would agree with it. In the absence of evidence on this score, any current interpretation must rest on the arbitrary selection of material, and to some extent on the 'personal equation' of the writer. Nevertheless the suggestion which I offer here is, in my view, far more culturally congenial than the contention that the design is a Medusa: and it is more in line with what we know of warfare conventions in other parts of New Guinea.

Although the Trobriand Islanders must have changed considerably from what they were in Malinowski's day, it is perhaps not too much to hope that many underlying values are still present. It should therefore be possible to test out what I have said here against the more recent research of Dr. Powell, when his material is available.

#### A Survival of an Ancient Middle Eastern Moustache Fashion. By P. Lenk-Chevitch. With four text figures

66

A curious way of wearing a moustache is met with among some country people of the North-Western Province, Baluchistan and Sind in West Pakistan. The moustache is cleanly shaved under the nose, often leaving some hair on the sides of the upper lip (fig. 1). This striking way of adorning the face appears to be very anachronistic in the present surroundings. Questioning of local people produced no satisfactory explanation of its origin. They say that it is an old Muslim custom, but this was found to be incorrect, as the Prophet



FIG. 1. MOUSTACHE WORN IN SIND, 1956  
*The shaved part is sometimes much wider*

ordered only that the moustache should be trimmed, not that it should be shaved.

Some years afterwards, I chanced to see in the Museo Nazionale Romano (Baths of Diocletian), a Roman sculpture called 'Head of a Dying Persian' (fig. 2); it represents a warrior wearing exactly the same moustache as do some of the contemporary country folk mentioned at the beginning of this note.



FIG. 2. ROMAN HEAD OF A DYING PERSIAN

Further investigation showed that this fashion subsists among Kurds in Western Persia and was also found in Turkestan not long ago.

In this article I shall attempt to trace the origin of this custom. Fortunately its occurrence in antiquity is testified by numerous documents, some of which I cite hereunder.

Fig. 3 shows a head bearing a similar moustache, from a wool fragment of Parthian origin from Nom Ula (Kozlov Collection, Hermitage Museum, Leningrad), dated to the first century B.C. or A.D.<sup>1</sup> A bronze head of Parthian origin in the State Museum at Berlin shows the same moustache.<sup>2</sup> The Sassanid King of Persia, Shapur II (A.D. 310-379), used to shave his upper lip in the same way.<sup>3</sup> In Pakistan itself, we find the fashion persisting in some of the excavated monuments. At Taxila, it occurs on a terra-cotta head of Vajrapani<sup>4</sup> (fourth-fifth centuries A.D.), of the Buddhist period and belonging to the Indo-Afghan school. In this connexion, we must not forget that the remains of an Iranian temple of the first century B.C. have been excavated at Taxila.<sup>5</sup>



FIG. 3. MOUSTACHE SHOWN ON A PARTHIAN WOOL EMBROIDERY



It is possible to question whether the Parthians borrowed this fashion from the region which is now West Pakistan, or whether they introduced it in the course of their penetration and invasions, which were almost uninterrupted from the time of the Achaemenian Kings. According to the *Periplus of the Erythraean Sea* (first century A.D.), the lower Indus valley (Sind) was occupied by numerous Parthian principalities.



FIG. 4. A PRIEST OF MOHENJO-DARO (c. 2500 B.C.)  
Note the shaved upper lip. After Marshall

Several bas-reliefs of the Achaemenid period (c. 400 B.C.) show the same moustache, as on the Apadana stairs in Persepolis.<sup>6</sup> In early Sparta (c. 800 B.C.), the ephors used to order the citizens 'to shave their upper lip.'<sup>7</sup> The custom was observed in ancient Syria and at a much earlier date in old Sumer and in Mari (3000 B.C.).<sup>8</sup>

Ancient Egypt was an exception and never adopted the fashion; this fact may throw light on human relationships in the old world.

Finally we reach the Indus civilization of Mohenjo-daro in Sind. Most of the heads excavated there had their moustache shaved. The well-known bust of a priest of Mohenjo-daro, one of the best preserved, may serve as an example (fig. 4). Others are reproduced by Mackay<sup>9</sup> and Marshall.<sup>10</sup>

This fashion is not, however, used any longer by the inhabitants of the Mohenjo-daro region or of the surrounding districts, and this may explain why it has not been noticed by the excavators of Mohenjo-daro. Further to the north and to the south about 10 per cent of the population adhere to this mode.

In conclusion, I suggest that this fashion, probably the oldest extant on earth, began at Mohenjo-daro towards 3000 B.C., spread to Sumer, Mesopotamia, Syria and Greece, lingered for a long time in Persia and survived to the present day in Sind, Baluchistan and the North-West Frontier Province in Pakistan, and among the Kurds and some tribes of Persia.

#### Notes

- <sup>1</sup> A. U. Pope, *A Survey of Persian Art*, London, 1938, Vol. I, p. 691, fig. 240.
- <sup>2</sup> *Ibid.*, Vol. IV, Plate CXXXIV, c.
- <sup>3</sup> *Ibid.*, Vol. I, fig. 211.
- <sup>4</sup> Sir John Marshall, *Taxila*, Cambridge, 1951, Vol. II, pp. 468 and 472; Vol. III, Plate CXXXVIII.
- <sup>5</sup> Pope, *op. cit.*, Vol. I, p. 445.
- <sup>6</sup> *Ibid.*, Vol. IV, Plate XC (second human figure from left).
- <sup>7</sup> J. B. Bury, *History of Greece*, London, 1952, p. 125.
- <sup>8</sup> J. Pritchard, *The Ancient Near East*, Princeton, 1954, figs. 21, 24.
- <sup>9</sup> E. J. H. Mackay, *Further Excavations in Mohenjo-daro*, Delhi, 1938, Vol. II, Plate LXXIV, figs. 23, 24, 25, 26, Plate LXXVI, fig. 18.
- <sup>10</sup> Sir John Marshall, *Mohenjo-daro*, London, 1931, Vol. III, Plate XLIX, figs. 4, 5; Plate XLVIII is reproduced in our fig. 4.

## CORRESPONDENCE

### The Ostrich in South-Western Asia: A Further Note. With a text figure

67 SIR.—In 1928 during excavations in Mound 'W' by the Field Museum—Oxford University Joint Expedition to Kish, Iraq, we found a limestone plaque (fig. 1) showing an ostrich being ridden about 550 B.C. This remarkable scene (C.N.H.M.



FIG. 1. A LIMESTONE PLAQUE FROM IRAQ

Neg. No. 75314) is believed to be unique; additional information on similar data would be most welcome.

During the same year (*Illustrated London News*, p. 567, 7 April, 1928) Count P. Guerrini Malmignati of the Italian Consulate in Damascus described chasing six ostriches by car in the North Arabian or Syrian Desert. Four birds were shot and two were caught by the neck by Sheikh Midghem from the racing car.

Professor A. Aharoni (*Science News Letter*, p. 45, 18 July, 1931) described a similar chase near Qaryatein between Damascus and Palmyra. Four ostriches were chased: one escaped into the mountains; one was shot; and two were caught by the neck from the car after being pursued at high speed for two hours across the desert.

It is little wonder that the ostrich has almost disappeared from the Arabian Peninsula (*MAN*, 1952, 73).

Peabody Museum, Harvard University

HENRY FIELD

### The Study of Race Relations. Cf. *MAN*, 1957, 145-148, 224, 245

68 SIR.—Dr. Freedman certainly contributes 'to help reinforce the contention that race relations is not a distinct discipline with its own technique.' His analysis is muddled. What for example does he mean by the statement that the study of contact between physically distinct races 'is morally wrong as well as being scientifically absurd'? As scientists we concern ourselves with facts, not with value or emotion, so that such statements are ammunition for those who charge that there is no science in social science.

Although Dr. Freedman has suggested that we may 'treat the race of race relations as fortuitous,' and that the scholar must remove 'the misconception that because there is a subject called



race relations there must be something special about the relations between the physical anthropologist's races,' he promptly defines colonial situations as one where *élites* 'define themselves racially.'

Again, Dr. Freedman asserts, correctly, that 'it is in the nature of all society to differentiate itself into units which stand opposed to other units.' Why does he suggest, or try to suggest, that racial differences are, or should be, excluded from such differentiation? Is Mr. Richmond's analysis of the colonial people in Britain 'morally wrong as well as being scientifically absurd'? Of course, we all share Dr. Freedman's warning that research must not be limited to investigating relations between whites and others. His example which follows does not, however, take us far from this limitation as American Indians and American Negroes are quite distinct racial groups.

Dr. Freedman suggests that racial criteria, which we thought that he had discarded, 'assist' in the 'grading in social status' rather than 'ascribe' individuals 'to relatively fixed categories.' Truly,

there is flexibility and the differentiation produced by racial criteria can be overcome.

Finally, Dr. Freedman suggests that 'it is methodologically unsound to treat socially recognized discrimination as though it were constantly under the review and control of individual minds.' If it is socially recognized, discrimination, presumably based preponderantly on racial criteria, has regularity and can be predicted.

Students of race relations will share Dr. Freedman's view that what has been achieved to date 'is less open to debate' than what could be done in the future. I have no contribution to make to alter this balance but I am equally conscious of the fact that Dr. Freedman is in the same boat.

Mr. Morris, in his contribution to the symposium, has conducted a factual analysis and hence made a profitable start so far as East Africa is concerned.

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## REVIEWS

### GENERAL

**Archaeology and its Problems.** By S. J. De Laet. London (Phoenix House), 1957. Pp. 136, 21 plates, 3 text figs. Price £1 1s.

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This short book, written by one of Belgium's leading archaeologists, whose work is widely known outside his own country, is a most welcome addition to the growing literature dealing with the principles, practice and limitations of archaeological research. Originally published in French in 1954, it is here admirably translated into English by Mrs. Ruth Daniel.

For the English reader much of the earlier chapters, dealing with the nature of 'raw' archaeological evidence, its discovery and excavation, and the chronological problems which it poses, covers ground which is already familiar, thanks to the popularity which archaeology has enjoyed in Britain in recent years. In this matter of public interest and support our nearest continental neighbours are far less fortunate, as Professor De Laet is ruefully aware, and it is to stimulate in his own countrymen a more responsible attitude towards their antiquities, as well as to clarify his own thoughts on methodological problems, that he has written this manual.

It is in the last two chapters, on the limitations and possibilities of archaeological interpretation, that the author makes his most noteworthy contribution. It is high time, as he points out, that archaeologists began to take seriously the inherent limitations of the primary sources they use. These primary sources consist not of excavations themselves, but of the *published reports* of excavations, whose accuracy and reliability can never be checked directly, owing to the transitory and destructive character of excavation itself. It is thus inevitable that the primary sources of archaeological research are from the start tainted, in unknown degree, by the personal predilections and the varying skill and experience of individual excavators. For this short section alone, in which this fundamental matter is discussed in print for the first time, this book deserves to be read as widely as possible.

Hardly less valuable is the section which follows, in which the concepts of race, culture and language, as possible approaches to the interpretation of archaeological material, are critically defined and discussed. In the past, the confusion of these categories, and even more the deliberate attempt to equate them, has bedevilled the progress of prehistory; and even if archaeologists themselves now recognize the futility of such attempts at correlation (except within very restricted limits in time and space), the inability to distinguish these concepts one from another is still widespread among the growing public to whom archaeology appeals, and constitutes a definite barrier to the successful diffusion, in popular form, of the results of current research.

A wider understanding of these inherent limitations, by professional workers no less than by their public audience, is an essential condition for the further progress of archaeological studies. On the more positive side, as Professor De Laet reminds us in his final chapter, there are still huge potential reservoirs of untapped know-

ledge in the material at our disposal. To draw upon these, we must first formulate, as he does in outline here, the questions which we wish to ask. Only then can we develop the new techniques which will give us the answers.

R. J. C. ATKINSON

**A History of Technology: Vol. II, The Mediterranean Civilizations and the Middle Ages.** Edited by Charles Singer, E. J. Holmyard, A. R. Hall and Trevor I. Williams. O.U.P., 1957. Pp. lx, 802, illus. Price 8 gns.

70

The second volume of this mammoth work follows the high standard of material and presentation set by Volume I. At eight guineas, however, it is a book which few private individuals will be able to afford, although it will be an essential purchase for all reference libraries. The volume deals in the main with the classical civilizations of the Mediterranean region and the cultures which had developed in barbarian Europe but which generally had their origins among the Mediterranean countries. The dates given by the editors for the range of the work are from 700 B.C. to A.D. 1500—a sweep of time covering at its extremes the late bronze age and the end of the middle ages. This has meant in most cases a welcome linking of classical and mediaeval technology presenting an unbroken thread between the ancient world and modern Europe.

Another important aspect of the volume is the bringing together of archaeological evidence and the beginnings of literary evidence—many contributors in fact combine material from both sources. A veritable galaxy of scholars has contributed to the volume in the fields in which they are pre-eminent, D. B. Harden on glass, Rex Wailes on windmills and J. W. Waterer on leather, for example. With such a wealth of material it is hopeless to attempt any kind of critical study or even to single out the best for praise. The interests covered by the volume are extremely catholic and combine serious information for the expert with informative reading for the layman. Indeed, if readers' interests lie in such diverse subjects as the introduction of the arabic numeral, the building of King's College Chapel at Cambridge, Leonardo's screw-cutting machine, Columella's olive press or the scraping of tartar from a wine cask, there is something there to instruct and to please. Many of the authors have included illustrated material from the mediaeval psalters and other contemporary manuscripts with the result that the book contains an excellent representative collection of the efforts of the mediaeval illuminators.

The main impact of this volume is to show how much could be achieved in ages when slow and laborious work was combined with brilliant craftsmanship. It is indeed a sobering thought that many of the techniques common in pre-industrial Britain (and common in many lands today) have changed not at all in the course of their gradual spread through time and through the civilizations of



ancient Greece, Rome and Byzantium, and in some cases even Asia and China.

JOHN HIGGS

**Cambridge Papers in Social Anthropology: No. 1, The Developmental Cycle in Domestic Groups.**

**71** Edited by Jack Goody. C.U.P., 1958. Pp. vii, 145. Price 21s.

The central theme of this symposium, which consists of an introduction by Professor Fortes and four essays by social anthropologists at or associated with Cambridge, is indicated by its title. The contention is that in any society the domestic group (which it is useful to distinguish from the elementary or any other kind of family) and its social significance may best be understood when it is regarded as a developing institution, the form and functional significance of which change as it passes through successive phases from its inception (usually by a marriage) to its dissolution (usually by the death or dispersal of some of its members) and replacement by new units of the same kind. Of course everybody knows that in most societies people live in homesteads and have children who eventually grow up and disperse. But the systematic interpretation of domestic life as a process, and the consequent emphasis on its modes of segmentation and replacement, is shown by these studies to provide a new and useful framework for research.

Professor Fortes sets out this framework in a penetrating introduction, and the essays which follow make use of it in differing contexts and degrees. In the first, Dr. Freeman gives a straightforward account of family organization among the monogamous and cognatic Iban of western Borneo. Among them any child may inherit from either parent, and marriages are about equally virilocal and uxorilocal, depending on whether domestic pressures make marrying-out more expedient for bride or groom. Families divide when an adult sibling leaves home (generally because two elementary families in one homestead are one too many), but somebody always stays to take over the parental homestead. This is good, unpretentious ethnography, soundly based on numerical data. In the next essay Dr. Goody, after some discussion of lineage and segmentation theory, compares two farming communities in northern Ghana which differ only in that in one men inherit property in the male line, while in the other this is true only of immovable property, movables being inherited matrilineally. Making good use of statistical data, the author convincingly exhibits the structural implications of this difference for the size and composition of farming groups, the control of food reserves, and residence pattern, in the two groups. In the third study Dr. Stenning discusses the mutual dependence which subsists between a Fulani household and the herd which it owns: to maintain economic self-sufficiency there must be enough stock and there must be enough humans to look after it. Where the required equilibrium cannot be achieved (as in the first years of marriage) recourse is had to parental assistance, loan or temporary association with a related household. Though innocent of quantitative data Dr. Stenning's account contains much of interest, but his work would be easier to read if he would cultivate a simpler style of writing. One knows what is meant, for example, by the phrase 'the initial non-viability of a filial household may be completely nullified' (when the parents are well off), but nothing would be lost by saying it in plain English.

The concluding essay, unlike the others, is not a report on field-work. It is a brilliant *tour de force* by Dr. Leach, in which he shows, by a detailed and quite convincing analysis of Malinowski's own material, that the master was mistaken in supposing the several different meanings of the Trobriand word *tabu* to be quite unconnected. He explains at the same time why the Trobrianders have four clans. Dr. Leach's demonstration effectively combines the central notion that a Trobriander's attitudes towards his relatives change as he changes residence in childhood from father's to mother's hamlet, with the point (suggested 25 years ago by Hocart but practically ignored ever since) that the primary reference of many kinship terms is to categories of persons (which may be defined by common group membership or even by locality as well as in genealogical terms) rather than to individual kin defined by specific genealogical links.

In all, this symposium is to be welcomed as a significant and stimulating contribution to the study of the development of family groups through time. The next number of the series, on caste, will be looked forward to with interest.

JOHN BEATTIE

**Theory of Culture Change: The Methodology of Multilinear Evolution.** By Julian H. Steward. Urbana, Ill. (U. of Illinois P.), 1955. Pp. 244. Price \$4

**72**

In the main this is an edition of already published papers ranging from 'The Economic and Social Basis of Primitive Bands' (1936) to 'Evolution and Process' (1953), most of which are already too familiar to require comment now. Steward claims to have invented a 'coherent and unified' 'general method for determining regularities in the functional interrelation of culture patterns and in the processes of cultural change,' though he also states that his main interest is in 'the kinds of parallels and similarities . . . distinguished by their limited occurrence and specificity.'

I must report that I have not discovered any such general method in this book, though some of its essays certainly take one or two steps in that direction. Within the field of American anthropology Steward's notion of 'successive levels of socio-cultural integration' (1951), for instance, advances a little the general idea of Redfield's *Folk Culture of Yucatan* (1941) by not insisting on an organization—disorganization antithesis between such contexts. What, for the rest, Redfield's advocacy gained by being closely conjoined with a detailed ethnography of one particular culture area Steward's might gain also when the study of Puerto Rican culture (which he made jointly with Manners, Mintz, Padilla, Scheele and Wolf) is published in full, and not in résumé only as in the book under review. And when this appears we shall also be able to see if in some Puerto Rican peasant populations the inventing of a local identity to be set in opposition to the national one is by means of the promulgating of myths about that locality, in a symbolism which psycho-analysts are quick to identify with retrogression. A structural analysis of this kind of culture change, which is not confined to peasantries *per se*, of course, would extend the kind of thought about culture that Steward's exemplifies.

R. J. APTHORPE

**Modern Sociological Theory in Continuity and Change.**

Edited by Howard Becker and Alvin Baskoff. New York (Dryden Press), 1957. Pp. xiv, 756. Price \$6.50

**73**

The editors of this volume have assembled a group of contributors who trace developments in various fields of sociology from 1917 to the present. As is inevitable in such collections involving many authors and many points of view, it is not possible to extract any central or dominating focus that would give coherence to the book as a whole. The reader, however, no longer expects such coherence in volumes of collected essays, so he is, as a matter of fact, quite disarmed by the candour with which the editors themselves recognize and describe the discontinuities in the make-up of their own book and in the contents of the contributions.

What the editors seem to be saying in their preface is that the lack of a systematic focus in the work of their contributors reflects a lack of a focus on any coherent set of problems in the field of sociology itself. It would be unfair, however, for me to carry this interpretation too far. For the editors, in having noted the contemporary fragmentation of sociological thought and activity as one of the major characteristics of the field, deliberately set for their contributors the task of trying to bring some coherence to the vast literature and range of ideas that they bring under review. That, in the end, the book lacks a focus constitutes a criticism of sociology and not of the contributors or editors of the volume in their roles as contributors and editors.

Taking the book at its face value, it is divided into four major parts: the first considers contemporary major trends in theory and methodology including historical, structural-functional and sacred-secular theory as well as others; the second focuses on areas of specialized interest such as small groups, social disorganization, social stratification, sociology of knowledge, law, religion and art, literature and music; the third deals with the convergence of



anthropology, social psychology, and psycho-analysis with sociology; and the last contains special reports on sociological research and theory in Britain, France, Germany, Italy and Japan.

The individual contributions will, of course, have a different appeal to different readers according to their interests. For me, the report on sociology in Britain by W. J. H. Sprott, the report on cultural anthropology and sociology by C. W. M. Hart, 'The Range of Sociology of Knowledge' by Franz Adler and 'Social Stratification in Perspective' by Roscoe C. Hinkle were the studies that held the greatest interest. Becker and Boskoff have added original contributions of their own and these together with the other 20 chapters represent the work of an impressive assembly of contemporary sociologists.

ARTHUR J. VIDICH

**The Concept of Development: An Issue in the Study of Human Behavior.** Edited by Dale B. Harris.

74

Minneapolis (Minnesota U.P.) (London agents: O.U.P.), 1957. Pp. x, 287. Price £1 18s.

This includes 17 contributions to a conference concerned with definitions and terms. Several of the essays get near Hook's definition of development as change which has a continuous direction and culminates in what is qualitatively new. I find it difficult to restrict the term development to processes resulting in the qualitatively new, but welcome the view that an organ develops with a forward reference to future functioning, not under functionalist stimuli in its initial stages, e.g. a limb grows often from a bud of tissue. But mayfly which in the egg stage have not been exposed to the alternation of day and night do not develop the daily routine of the normal insect. Sensori-motor functions in lower animals and neo-natal humans give place to adaptive behaviour in higher animals and late human infancy, with increasingly pervasive effects. Marked changes are specially notable about the twentieth day in puppies and the ninth month in humans. Orthogenesis is a term that, in zoology, is almost forgotten, but it seems to be still in use among some psychologists, in spite of its danger of neglecting environmental and social factors. Much obscurity is due to averaging growth or achievement scores for groups of individuals of a particular chronological age. It is better to follow specified individuals through their individual developments, i.e. linear study is needed rather than the averaging of data from cross-sections of a population.

Dangers of unconscious tampering with or neglect of data that disturb an initially inferred sequence of development are universal, but are specially harmful when we study development in multi-factorial fields such as human institutions, literature and the arts. Conclusions rarely escape limitations due to the enquirer's preconceptions.

It is interesting to note that Lloyd Morgan's idea of Emergent Evolution, Smuts's Holism, and Boas's emphasis on local and temporal relativity in human cultures all seem to have won their place in scientific currency, and have become more or less anonymous.

H. J. FLEURE

**Exotische Masken.** By Gerdt Kutscher. *Plastik der Primitiven.*

75

By M. Heydrich and W. Fröhlich. Stuttgart ('Die Schönen Bücher'—Strache), 1953 and 1954. Each pp. 16, 48 plates. Price DM. 5.80

These two picture books are a welcome and useful addition to the 'literature' (if we may so call publications which are in essence pictorial rather than verbal) of primitive or tribal art, because, although they are both responses to the apparently insatiable demand for anthologies—in the first case from the collections of the ethnographical museums at Stuttgart, Cologne and Frankfurt, in the second from the Cologne collections alone—yet the pieces illustrated are not the usual selection from other people's anthologies but are for the most part unpublished, as well as worth publishing. Nevertheless, such publications can only whet the appetite for those more intensive and comprehensive monographs on the cultures of particular tribes and areas which are what scholars really need from the world's museums. How valuable, for example, would be a corpus of Cameroons sculpture in German museums! This would at once introduce order into a field in which we are all, in its absence, condemned to an embarrassing vagueness of utterance. Certainly

the learned compilers of these two slim books deserve our praise and not our blame; but we may still shed a tear for the price which the world of scholarship must pay for popular culture and the U.N.E.S.C.O. idea.

Both books maintain a remarkably high standard, even for these famous museums, in the quality of specimens chosen. Perhaps the least good, aesthetically speaking, is an enormous and monumentally ugly Benin bronze head of modern date (though here attributed, according to the untenable von Luschan—Struck hypothesis of Benin art history, to the sixteenth or seventeenth century); and even this can be justified in terms of world market values, since similar pieces fetch £1,000 or more. The documentation is generally good, about 100–200 words being devoted to each piece, although in some cases material and ascertainable facts are missing; e.g. of the three Yoruba masks in the mask volume, those on pp. 22 and 24 are for the Gelede Society, and that on p. 23 is for the Egungun Society and in the style of Abeokuta.

A notable example of economy of means is provided by the Gilyak woodcarving on p. 17 of *Plastik der Primitiven*, in which the deft removal of a few chips from a small cylinder of wood still bearing its bark admirably suggests the profound *gravitas* of a tribal elder statesman. This may be partly due to the excellent photograph, and indeed the photographs are very competent and dramatic throughout both books, in spite of a regrettable tendency not to show the whole of a specimen. The modern fashion for striking effects in photographing tribal art often introduces a subjective element which tends to mislead. My experience of African art had increasingly led me to the view that the many tribal styles of Africa are without any common denominator (unless it be the negative one of freedom from the European canons); but these books (and some others) almost persuade me that not only African but also American and Oceanic styles share a strong vein of German Expressionism.

WILLIAM FAGG

**Standard Dictionary of Folklore, Mythology, and Legend,**

76

Vol. II (J-Z). Edited by Maria Leach, with Jerome Fried. New York (Funk & Wagnall), 1950. Pp. (3), 533–1196

After Dr. Lindgren's preceding review (MAN, 1950, 175) it seems permissible to concentrate on a section and several articles of the second volume. Among the 'survey articles' Professor Archer Taylor's 'Proverb' and the general survey of folk 'museums' are very useful. The student of amulets will be disappointed, as neither the magician's wand nor the philosophers' stone are 'talismans.' The article on the 'philosophers' stone,' though very short, contains more errors and half-truths than can be corrected in a review. As protections against 'overlooking' horn-shaped amulets and fig-hands predominated over frog amulets or 'various loathsome creatures.' Under 'unicorn' the narwhal's tooth has not been mentioned once. 'Right and left' deals exclusively with an American square dance and the cross-reference to 'withershins' is missing. 'Lärmzüge' are ambivalent: the noise is meant to drive away demons and, at the same time, to awaken the vegetation.

The highlights of the Celtic section are Professor R. S. Loomis's concise and lucid contributions; but the numerous short and anonymous articles are also of a high quality. Under 'Samain' the Eve of the Celtic New Year might have been mentioned instead of the beginning of winter. The Irish god Nuadu, though repeatedly referred to, might have been given a heading of its own. A regrettable omission is an article on Celtic sea lore. The following entry occurs under 'key' (!): 'In Brittany the menhirs are said to be the keys to the ocean; if they were dug up the sea would pour up through the holes.' This statement may have a bearing on the significance of some menhirs at Carnac and other places threatened by encroachment of the sea (*Folk-Lore*, Vol. LXIII, 1952, pp. 231ff.). However distinguished the contributors of the *Dictionary*, the student could assess the value of such a statement only by finding out from the editor who wrote the article, and by then asking the author to reveal his sources. A lengthy and hazardous procedure.

Only after publication of the promised Index Volume will it be possible to pass final judgment on the 'representative sampling.' It is nevertheless safe to say that even then scholars will in the first



instance resort to Hastings's *Encyclopædia of Religion and Ethics* and not to this *Dictionary of Folklore*. ELLEN ETTLINGER

**The Eye Goddess.** By O. G. S. Crawford. London (Phoenix House), 1957. Pp. 168, text figs., plates. Price £2 10s.

**77** It is now generally recognized that, from neolithic times till the beginning of the Christian Era, south-western Asia exerted, not continuously but often, a strong cultural influence on Europe. This fact, however, does not exempt anyone who seeks to trace westward the transmission of a trait from showing first that he is dealing throughout with the same trait and secondly that the dates fit.

The *Antiquaries Journal* has recently (1957, pp. 131 ff.) published an admirable example of such a demonstration by Professor Hawkes and Miss M. A. Smith. They show that a particular type of bronze cauldron was being made in Urartu (Armenia) about 800 B.C., and that it found its way, via Greece and then Gaul or Spain, to Ireland, which it reached a little before 700 B.C. They point out that such cauldrons, used primarily for ritual purposes, play a prominent part in Celtic mythology, and suggest most plausibly that 'strong associations with the supernatural' came in with them.

Crawford's theory is that a fertility cult having as its principal feature the worship of an Eye Goddess came into existence in the Fertile Crescent at a very early period, and later on, from a focus in the region of the Upper Euphrates, spread to Greece, Italy and Spain. From Spain it spread north to Brittany and Ireland and south to the Canaries and West Africa. In support of his theories he adduces, from all over this area, statuettes, carvings and paintings which, he claims, represent the Eye Goddess.

Now in the first place it is by no means certain that all his 'eyes' are eyes. He warns non-experts against scepticism (p. 91), but admits (p. 98) that some designs which he has interpreted as eyes may be meant for breasts; others look more like stars, flowers or mazes. Even when they are clearly eyes it is often not evident that they are feminine.

His type figures are a number of statuettes found at Brak on the Upper Euphrates. They are very distinctive; their wide staring eyes (without other features) being represented in such a way as to make the figures look double-headed. If these set the fashion for Eye Goddesses one would expect to find them reproduced further west, but hardly any of his 'eyes' show the slightest resemblance to those of the Brak statuettes.

As for dating, Crawford's examples are drawn from all periods down to the present and, in spite of such chapter headings as 'On to Iberia,' anyone who tried to provide the Goddess with a timetable would find himself in difficulties.

His illustrations, many of them from recondite sources, are excellent and all that he says is interesting, but the existence of the Eye Goddess as a principal object of worship in three continents remains unproven. RAGLAN

**Essays in Linguistics.** By Joseph H. Greenberg. New York (Wenner-Gren Foundation), 1957. Pp. 108

**78** Professor Greenberg follows in direct line other earlier American anthropologists—one thinks at once of Sapir, Boas and Kroeber—who have achieved distinction in linguistic studies as well. He is by now well known to linguists, especially for his provocative and stimulating papers on the classification of African languages. In the present volume he presents eight essays which are self-contained and separate treatments of a number

of topics in linguistics. All are new; none has appeared before; but the knowledgeable reader will recognize in one an expanded and revised version of 'Historical Linguistics and Unwritten Languages' which Professor Greenberg contributed some time ago to Kroeber's 'Anthropology Today.'

Though two at least are somewhat technical and remote, as the author himself admits, all the essays in the first instance are clearly aimed in the direction of his fellow anthropologists and for them no doubt the four which deal with the interrelation of language and culture will have the most immediate attraction. But there is something for nearly everyone. Besides the anthropologist and, of course, the linguist proper, the logician, the psychologist and the mathematician (whether they ultimately approve or not) cannot fail to find interest in Professor Greenberg's tentative explorations of the borderline areas between their disciplines and his.

In view of Professor Greenberg's recent comparative work, most Africanists on the other hand will especially wish to read Essays III and IV which discuss problems of linguistic relationship and classification. In these is adumbrated much of the theory which underlies and informs the practical comparative work resulting in the recent *Studies in African Linguistic Classification*. A list of titles is given to indicate how widely Professor Greenberg has cast his linguistic net: Language as a Sign System; The Definition of Linguistic Units; Genetic Relationship among Languages; The Problem of Linguistic Subgroupings; Language and Evolutionary Theory; Language, Diffusion, and Migration; Structure and Function in Language; Order of Affixing—A Study in General Linguistics. *Essays in Linguistics* is topical and recommended reading for all who are interested in language and the techniques of language-description.

J. BERRY

**Nature into History.** By Leslie Paul. London (Faber), 1957. Pp. 201. Price £1 1s.

**79** The author has read fairly widely and writes in a lively sentimental fashion about early man. Palaeolithic cave painters and Australian aborigines are probably far from primitive. The highest dating for cave paintings is 40,000 years ago but an age well below 20,000 years seems more probable and is more widely accepted. This is the most recent tenth of the probable period of *Homo sapiens* and perhaps the last twenty-fifth of the period of *Homo* in the more general sense. The elaborate social structure and rituals of Australians and those supposed to have characterized the cave painters are thus, in time, far from the primitive, and may show specializations by minds not absorbed in techniques of agriculture. The author enjoys castigating suggestions of earlier writers, and seems to look for proofs. He should remember that investigators in natural history do not prove things, they make tentative inferences that give a more comprehensible order to series of observations and that promote further enquiries. Moreover, serious enquiry in this field began barely 100 years ago, and is limited by dependence on finds, so we are still in the pioneering stage. Already, we see that schemes of inheritance, with a new combination of genes preceding each birth, promote rather than hinder variation, while weeding out less viable variants.

Mr. Paul speculates interestingly about language, which is one of the most fundamental human characters. Most workers think that rudiments of language as well as society may be heritages from pre-human forebears, but Mr. Paul leaps into the statement that man, society and language all began together through an act of special creation!

H. J. FLEURE

## AMERICA

**Die sozialökonomischen Verhältnisse bei den Azteken im 15. und 16. Jahrhundert.** By Friedrich Katz. *Ethnogr. Archæol. Forschungen* 3, 2. Berlin (Veb. Deutscher Verlag der Wissenschaften), 1956. Pp. 166

**80** This scientific study by Dr. Friedrich Katz of Vienna is of great importance and interest because it is based on a thorough and critical study of original sources. Dr. Katz gives a clear account of the social problems of life in Aztec society in the fifteenth and sixteenth

centuries. He aims to approach important economic problems previously not studied, e.g. the tribute (income of the state and use thereof), the importance of commerce, significance and place of handicraft in the nation's household and the economic base of the Aztec town. Although thoroughly studied questions concerning the property of the soil, the competence of the rulers, the existence of nobility, legislation, relations and military tactics have already been published, Dr. Katz amplifies lacking points. In this connexion he



mentions the historic cohesion of different problems: power and competence of the ruler, and causes of war. He succeeds in throwing new light on many of these points and criticizes old conceptions uttered by Bandelier. Katz considers that the *calpulli* were not exogamic clans and that the ruler was not subordinated to the authority of the tribe. As to the military organization Katz also disagrees with Bandelier.

In his conclusion he points out that the discrepancies in old reports of the sixteenth and seventeenth centuries are due to the very abrupt changes in Aztec life, in their turn due to the rapid growth of the social organization to its highest point in less than two centuries.

GUDA E. G. VAN GIFFEN-DUYVIS

**A Survey of the Aboriginal Populations of Quebec and Labrador.** By Jacob Fried. *Eastern Canadian Anthropol. Ser., No. 1.* Montreal (McGill U.), 1955. Pp. iv, 121. Price \$1.50.

81

The material presented within the covers of this publication is the result of a seminar held at McGill University and initiated by Fried in the late summer of 1954. The attendance at the seminar was drawn from a wide variety of sources, including, besides the departments of anthropology, sociology, zoology and geography at McGill, scholars from the Arctic Institute of Canada, the Montreal Botanical Gardens, the University of Montreal and the University of Ottawa. Assistance of one sort or another was given by the Royal Canadian Mounted Police, the Hudson's Bay Company, the Indian Affairs Branch and many others, whilst publication was made possible by grants from the Graduate Research Committee of McGill and the Research Bureau of the Province of Quebec.

The aim of the publication is to provide 'practical and empirical' information of a sort which will afford 'a general orientation.' The material itself is organized under four headings, viz. (1) 'a comprehensive bibliography and an analysis of the published literature,' (2) three brief papers discussing archaeology, linguistics and physical anthropology as they apply to Quebec and Labrador, (3) an ethnographical map with supplementary data on statistics, location, languages, etc., and (4) an appendix listing 'Sources of Information' and 'Members of the Seminar.' The bibliography, while possibly not absolutely complete, is nearly so, and will be an immense help to any student. The brief contributed papers are likewise most helpful, although one might take exception to some of the statements made and to some of the implications; e.g. that the Quebec Indians probably arrived not more than 2000 years ago (Lowther); that the Baffin Island Eskimo possess some 'primitive' characteristics such as receding forehead and chin and heavy supra-orbital ridges (Radwan-ski); or that 'although the Algonquians probably were the equals of the Iroquois in bravery, intelligence and physical stamina, they did not have the same determination, strength of character and spirit of organization' (Lavolette).

Despite any minor flaws or omissions inevitable in a survey of this kind, the present one will undoubtedly become the *vade mecum* of every anthropologist concerned with Quebec and Labrador for quite some time to come. It will be regarded as a sort of pocket encyclopedia to its subject, and we could only wish, with that in mind, that it were of half the dimensions so that it would the better fit into the pockets waiting to receive it. KENNETH E. KIDD

**A History of the Ancient South-west.** By Harold Sterling Gladwin. Portland, Maine (Bond Wheelwright), 1957. Pp. 383, illus. Price \$8.50.

82

This is a beautiful picture book of South-western pottery types; and it is creditable to the intelligence of the lay public in the United States, that readers to whom the author's facetious style is congenial should be interested in archaeological evidence and a historical argument. Rejecting the more popular drought erosion theory and concentrating on the Athapaskan invasions, Mr. Gladwin constructs a military history of the South-west from the archaeological material, century by century and even decade by decade. Those who do not accept his theories may nevertheless enjoy his fine illustrations.

BARBARA AITKEN

**The Indian in Modern America: A Symposium Held at the State Historical Society.** Edited by David A. Baerreis. Madison, Wis. (State Hist. Soc.), 1956. Pp. xxii, 70. Price \$2.

83

Here is an important little book for those interested in the present position of the American Indian in the United States. In it E. A. Hoebel gives the only account known to me of the Indian's legal position *vis-à-vis* the Federal Government. The legal situation is so obviously crucial to studies of culture contact that one wonders how it can so often be neglected. It is to be hoped that this chapter represents a preliminary effort and will be reproduced later on a much expanded scale. W. W. Beatty furnishes a valuable review of 'Twenty Years of Indian Education,' and L. Barnett and D. A. Baerreis provide a paper pointing out that American Indians in the United States, despite cultural differences between them and local variations in the manner and degree of their acculturation, are homogeneous in their sharing of minority-group status. The rather detailed examples given in this last chapter are drawn from Wisconsin Indians.

It would not be difficult to write a review of this book half as long as the book itself. Failing this, one must be content with the briefest notice and a hearty recommendation. MARIAN W. SMITH

**Dawn in Arctic Alaska.** By Diamond Jenness. Minneapolis (U. of Minnesota P.) (London agents: O.U.P.), 1957. Pp. 222. Price £1 18s.

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An anthropologist must be prepared to work under difficult climatic conditions, but there are probably few, if any, who within a year have experienced a change in their field of operations from the steaming jungles of New Guinea to the barren tundras of arctic Alaska. That is exactly what the well-known Canadian anthropologist, Dr. Diamond Jenness, did when in 1913 he joined the Stefansson Arctic Expedition; and apparently he did not mind the change, because in the book which he has written about his first year as an arctic explorer he not even once complains about the cold, the darkness, and the hardship of life in the Arctic—and he had his full share of it all. For most of that year Dr. Jenness was left to himself, with the Eskimos, who live scattered along the unfriendly, northern coast of Alaska, as his only companions. Short of white man's food and with a canvas tent as his only protection against sub-zero temperatures and snowstorms he had to live with the Eskimos as an Eskimo in their small, crowded, primitive huts, sharing their often very sparse, undiversified and to a white man not always palatable food.

Although his first year in the Arctic must have been rather unpleasant Dr. Jenness was not discouraged but stayed on and became one of the leading scholars in the field of Eskimo ethnology and archaeology. This first year had undoubtedly something to do with it, because during that time he learnt to know the Eskimos intimately, learnt to respect them and to admire their ability to make a living in a part of the world where people without their ingenuity and hardiness would succumb. Dr. Jenness's book is as exciting as a novel and at the same time it gives a vivid and profound picture of the Eskimos in arctic Alaska at a time when they were still largely dependent on their ancient, admirable culture. Now we are eagerly awaiting an account of the next year. HELGE LARSEN

**Cultural Plurality and the American Idea.** By H. M. Kallen. Philadelphia (U. of Pennsylvania P.) (London agents: O.U.P.), 1956. Pp. 208. Price £2.

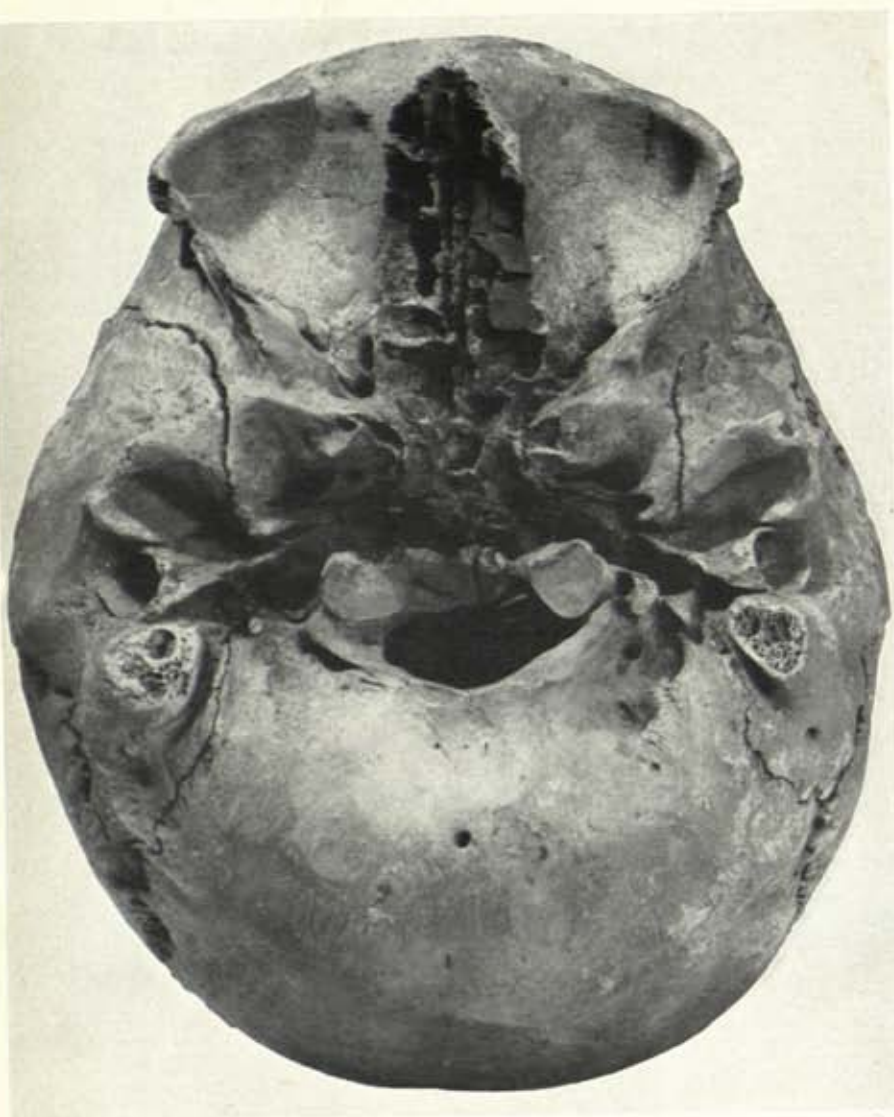
85

The author, who is a professor of philosophy, has set himself the problem of how far Americans can differ culturally from other Americans without ceasing to be good Americans, but though he has discussed it with many wise saws and modern instances, he cannot be said to have solved it. His essays are commented on by nine other writers, generally with approval, but there are criticisms that he is too vague, that he mistakes general problems for purely American ones, and that he seeks to make Americanism a surrogate religion. In his 'reprise' he is still somewhat nebulous, but ends with an eloquent plea for intellectual liberty. RAGLAN









(b)

(a-c) *The Romano-British skull (E.118.478)*

(a) *Norma basalis. The plane of the 'foramen magnum' makes a slight angle with that of the photograph.*

(b) *Norma verticalis.*

(c) *Norma lateralis.*

(d) *A Nubian skull (R.C.S.1.2280), norma lateralis, showing a more normal type of atlanto-occipital union.*



(d)

# CONGENITAL ABSENCE OF THE BASI-OCCIPITAL IN A ROMANO-BRITON



# CONGENITAL ABSENCE OF THE BASI-OCCIPITAL IN A ROMANO-BRITON\*

by

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**86** The specimen here described (Plate Ga-c) is one of a large series of Romano-British skulls now housed at the British Museum (Natural History). It was originally one of the Oxford Collection (No. E.118.478) which has been transferred. Although the anomaly is particularly rare, no detailed description has been made available and the catalogue merely records that it is deformed.

The calvarium is from a Romano-British burial at York from which no other remains would appear to have been saved. The individual was very probably a young adult male, although lack of obliteration along the suture lines is the only criterion available for aging. The mastoids are well formed, and the superciliary ridges are of adult size but more central than is usual in this British group. Although the external occipital protuberance is of normal size for a male, the nuchal lines are poorly developed, the bone being fairly smooth and rounded in this region.

The atlas vertebra has completely fused with the exoccipital and the supraoccipital areas of the bone, and this has compensated to some extent for the extreme platybasia. The basi-occipital, however, is completely missing, and a thin reduced anterior arch of the atlas is all that separates the false 'foramen magnum' from the body of the sphenoid which is normal. The posterior aspect of the sphenoid body suggests that some form of cartilaginous 'synchondrosis' may have been present between this bone and the atlas.

The general form of this occipital invagination is very similar to the more common type, which may result from such conditions as Osteogenesis imperfecta, Rickets, Hyperparathyroidism, and Paget's disease. In this more common type, however, this depression results from the plasticity of the bones and is not associated with such a specific congenital anomaly.

The changes resulting from this abnormality are of particular interest. The atlas has not only fused asymmetrically but is also modified, so that the left side of the vertebra is not so completely assimilated as the right and stands out more prominently from the base. The anterior tubercle has failed to develop or has been resorbed through lack of muscular influence, and the facet for articulation with the odontoid process (which may also have been modified) is directed ventrally. Fusion of the posterior arch and articular area of the atlas is associated with the reduction of the foramen transversarium and transverse process on the left side and possibly its complete absence on the right, though the latter area is slightly broken.

An uncommonly large foramen, measuring 2.5 millimetres in width, is present on the ventral surface of the occipital, anterior to the external occipital protuberance

and 23 millimetres from the opisthion. The increase in size of this foramen often seems to be associated with certain developmental anomalies such as oxycephaly, but the cause is at the moment obscure.

It is of particular interest to find that the sphenoid has undergone no noticeable change. Some compensatory growth could have taken place in this bone and the ethmoid, although the dimension from nasion to sphenobasion is still within the normal range. The measurements for this and other areas are given in Table I.

TABLE I. MEASUREMENTS OF THE ROMANO-BRITISH SKULL

Dimension	E.118.478	Romano-British mean§
	Millimetres	Millimetres
Max. Sagittal L. (L)	187	187.7 (14)
Max. Horiz. B. (B)	158.5*	141.4 (14)
Min. Front. B. (B')	92	97.2 (14)
Max. Front. B. (B')	129	123 (9)
Basio-Breg. Ht. (H')	100†	137.1 (14)
Basio-Nasion L. (LB)	83	102.5 (13)
Nasio-Breg. Arc (S <sub>1</sub> )	134	132.8 (12)
Breg.-Lambda Arc (S <sub>2</sub> )	137	130.1 (13)
Lambda-Opis. Arc (S <sub>3</sub> )	130‡	122.5 (14)
Nasio-Breg. Chord (S' <sub>1</sub> )	114.9	114.7 (13)
Breg.-Lambda Chord (S' <sub>2</sub> )	123	116.4 (13)
Lambda-Opis. Chord (S' <sub>3</sub> )	96.3	99.7 (14)
Biasterionic B. (Blast. B)	117.6	115.6 (13)
Breg.-Asterion Arc (P <sub>3</sub> )	177	170.1 (10)
Lambda-Asterion Arc (O <sub>7</sub> )	105	102.1 (10)
Simotic Chord (SC)	9.9	10.1 (9)
'Foramen mag.' L. (FL)	31.6	37.8 (13)
'Foramen mag.' B. (FB)	31.7	31.6 (12)

\* The greatest breadth has been taken above the squamo-parietal suture, even though this is not quite the maximum width.

† Taken to the 'pseudo-basion' on the ventral aspect of the anterior arch of the atlas.

‡ Taken to the 'pseudo-opisthion' of the atlas.

§ All but P<sub>3</sub> and O<sub>7</sub> were taken from data given by Goodman and Morant (1940) for a Belgic war cemetery.

As can be seen, most of the dimensions are quite normal for Romano-British skulls. Indeed, it is surprising that they were not more affected, and this suggests that Björk (1955) was right in thinking that growth in this area of the base is not so important in basal development as a whole, or for that matter, in other longitudinal growth movements, as previously thought.

Although the basi-occipital absence and basal invagination have resulted in a reduced basio-bregmatic height and basi-nasal length, there has been a compensatory increase in breadth. This breadth increase is particularly noticeable in the bi-parietal measurement, and even the squamous part of the temporal seems to take part in this bulging. It is particularly interesting to note that the posterior aspect of the vault somewhat resembles that of the Classical Neanderthal skull from La Chapelle-aux-Saints, and one wonders to what extent variation in vault

\* With Plate G and a table



growth of the basi-occipital region may have produced the notable width in this early group of man.

The false 'foramen magnum' is shorter than average, but the width is normal. The assimilation of the atlas is of course not extremely uncommon, and numerous authors have described this condition without any associated anomaly (Plate Gd). Brailsford (1945) notes that in the assimilation of the atlas there is nearly always a foramen on each side, between the posterior arch of the fused atlas and the occipital bone, for the passage of the vertebral artery and the cervical nerves, and this was found to be the case in the Roman skull.

It is evident that there would be very noticeable head-posture deformities resulting from this anomaly, for even in simple cases of atlanto-occipital union, as discussed by Dubreuil-Chamberel (1924), the head was displaced from its normal posture. It also seems probable that there would be associated deformities in at least other vertebrae.

#### Acknowledgments

I am indebted to Dr. K. P. Oakley, F.B.A., of the British Museum (Natural History) for access to the osteological material. I am also grateful for assistance from the Photographic Section of the same Museum.

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## MAE ENGA TIME-RECKONING AND CALENDAR, NEW GUINEA\*

by

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**87** Mae Enga are sedentary gardeners inhabiting the upper Lai and Ambum valleys in the Wabag Administrative Subdistrict of the Western Highlands District of the Territory of New Guinea.<sup>1</sup> They form one section of the numerous Enga-speaking people, and number some 30,000. By New Guinea standards, the population is dense, ranging from about 100 to 200 persons per square mile. Mae are divided into exogamous, named patrilineages, each comprising from about 100 to 1,000 members. Clans are segmented into named subclans and lineages, and are in turn segments of named phratries. Each clan occupies a defined, named locality; and competition for land has always been an important cause of the interminable inter-clan feuds.<sup>2</sup> Government patrol posts were first established in this area in 1939.

Sweet potatoes are the staple crop; but taro, bananas, beans, leaf vegetables, and introduced corn and cabbages are also important. The forests produce little food other than pandanus-palm fruits. Hunting of the few birds and small animals is undertaken mainly for sport and ritual purposes. Pigs and introduced fowls are raised; but the people are usually meat-hungry. Mae gardens and settlements occupy valleys and mountain slopes ranging from 6,000 to 8,000 feet above sea level. Above 7,000 feet, winter frosts regularly and severely damage sweet-potato gardens. The soil is poor, and expert gardening (with composting) is necessary to maintain adequate food supplies. The average annual rainfall is 108 inches, varying from 92 to 124 inches.<sup>3</sup> Rain falls on an average on 265 days of each year; but there

is a well marked division between the summer wet and winter dry seasons.

Mae are industrious people, both in tilling their gardens and in organizing inter- and intra-clan gatherings for magico-religious rituals, funerary and other distributions and exchanges of wealth,<sup>4</sup> and house-building.<sup>5</sup> For all these activities to be efficiently co-ordinated, they need some means of consistently marking the passage of time; and this exists in their luni-solar calendar in which the sequence of synodic months is correlated with seasonal activities by adjustment to observed variations of the ecliptic.

Their observations of intervals shorter than a month, however, are less systematic. Daily activities are roughly regulated in terms of the sun's transit. People can thus judge when they should leave home in the morning to work in their gardens, when they will eat after about half the intended work is done, when they should leave the gardens to reach home before the afternoon rain falls. These times are not standardized, and not all are specifically named. The commonly used terms are *koru* (day), *ku:ka* (night), *ju:jonggam* (forenoon, from about midnight onwards), *koraka* (sun at its zenith), and *aremanda* (afternoon, up till about midnight). Finer gradations can be made within this framework by referring specifically to sunrise and sunset, false dawn and the period immediately following dawn, and so on; but I rarely encountered such usages in everyday activities. A frequently used, discrete unit of time is that referred to as the time it takes to cook the contents of a ground oven. This cooking period is in fact most consistent. Of the dozens of cookings I timed, none

\* With a text figure and a table



varied more than 15 minutes either way from two hours. The Enga timing seems to be based solely on the experienced judgment of old men, who apparently need no external cues.

There appears to be no division of the night hours. Movements of the moon and stars are not used as guides, except that possum-hunters usually wait for moon rise before starting their search. Indeed, Mae are quite incurious about the stars; no constellations, for example, are distinguished. Nowadays, when an early start is desired for the day's work, the crowing of cocks is taken to indicate the approach of dawn. The first crowing is the warning, the second the signal to arise.

In the past, Mae did not recognize named, recurring intervals comparable with European weeks; but a few Mae today employ (fairly accurately) the pidgin English terms *sande*, *mande*, *tunde*, *trinde*, *porude*, *prainde*, and *sarete*. The term *sarete* is also occasionally used to denote a week. There are, however, local terms which enable Mae to indicate up to six days hence, or back to seven nights ago: *indupa* (today), *indupa kuka* (tonight), *taera* (tomorrow), *taera kuka* (tomorrow night), *kuwaka* (yesterday), *kuwaka kuka* (last night), *alembu* (day before yesterday), *alembu kuka* (night before last), *nduma* (two days hence or three days ago), *nduma kuka* (three nights hence or three nights ago), *ndumalje* (three days hence or four days ago), *ndumalje kuka* (four nights hence or four nights ago), *ndumakona* (four days hence or five days ago), *ndumakona kuka* (five nights hence or five nights ago), *apukona* (five days hence or six days ago), *apukona kuka* (six nights hence or six nights ago), *apuralje* (six days hence or seven days ago), *apuralje kuka* (seven nights hence or seven nights ago).

If an activity is planned for a specific day in the near future, those interested may tie the appropriate number of knots in *Cordyline* sp. leaves, or lash twigs across a stick, one knot or twig being removed as each day passes. Nowadays, some men count off the days according to the pidgin English week. If it is not a matter of nominating a specific day, but rather a period of several days in the near future, phases of the moon (*kana* or *ware*) may be used as a guide: *kana rasinggi* (new moon), *jakana pinggi* (waxing moon), *uwa pinggi* (half moon), *mbura pinggi* (full moon), *uwa pinggi* (half moon, waning), *rasio kiringgi* (waning moon), *kana kuminggi* ('dead' or absent moon). A woman can, for example, roughly calculate the onset of her menses in terms of lunar phases. If she menstruated at the last waning moon, she expects to do so again at the next.

The Mae month (*kana*, also meaning moon) is synodic, and I encountered no attempts to calculate it from one full moon to the next. The number of days in a month is not reckoned. Months are used as 'discrete' temporal indices as well as in calendrical sequence. The time of a sow's farrowing, for example, is taken to be just on four months from the time (lunar phase) when a boar was seen to serve the sow. This is a fairly accurate rule, for the usual gestation period is 112 days. Similarly, a pregnant woman informs her husband when she has missed her second menstrual period, and they expect the child about seven months after this.

The garden cycle is also calculated in terms of months. Depending on the altitude of the garden and on the variety of sweet potato planted, the time from planting to harvesting (*ea kana*, garden months) is from eight to ten months. There is also a short garden cycle (*m'ea kana*), ranging from two to four or five months estimated on the growing times of beans, corn, leaf vegetables, etc. Such indices are occasionally and loosely used to date the occurrence of projected activities: for example, a trading trip to the salt springs may be expected 'before this garden cycle is complete.' But normally, such long-term intentions are calendrically arranged. Enga in contact with Europeans now apply the term *ea kana* to the European year, counted from one Christmas to the next. They know from observation that Christmas is a regularly recurring festival. From this application, some have gone on to equate *ea kana* with the Enga year of 12 or 13 synodic months; but I am fairly sure that in pre-European times, the cycle of calendrical months as such was not so named.

Table I lists names of months, translations of names where these can be given, alternative names where these occur, times of the year about which they usually fall, relevant meteorological data, and the sorts of events normally correlated with particular months.

Correlations among seasons, events and months indicated in the table should not be rigidly interpreted. Although gardening is most intensive during the dry season, Mae also make individual gardens at any time during the year. Fights may occur at any time; but Mae explicitly state that there are more in the wet season because food is short, and men have little gardening to do and become bored. Men who get their main gardening finished early may take advantage of the last of the fine weather to initiate trading trips. On the other hand, hunting and eel-trapping are not really feasible outside the dry season; pandanus does not ripen in winter; house-building is seldom a wet-weather task because wet grass should not be used for thatching.<sup>5</sup>

Not all men are conversant with the details of the calendar; women know little or nothing of them. As a rule, two or three older men in each clan take it upon themselves to be the authorities for their kinsmen; but no specific ritual duties devolve on them. They are not, for example, the equivalent of Trobriand garden magicians, and have no power to induce other people to organize their activities in accord with the calendar. A few of the important, middle-aged men usually know a little of the 'correct' sequence of months and of intercalation; but these eventually receive the relevant knowledge from the elders and informally take over the latter's task. Young men rarely know more than a few of the months' names.

Furthermore, the list of months' names is not necessarily fully consistent from clan to clan, although it is consistent within a clan and tends to be so within a neighbourhood of clans. The list given here is in use among Kara clansmen at Sari; but the alternative names presented occur among several nearby clans. Some clans put *wambu* before *kumba*, some substitute *liu* for *ni*, and *aringgi* for *liu*, making *aringgi* distinct from and prior to *kumba*. Such discrepancies, however, are minor, and can be resolved by discussion.



TABLE I. THE MAE ENGA CALENDAR

APPROX. TIME	NAME	RAINFALL		USUAL EVENTS
		inches	days	
January	wambu-mupa (first-firstborn)	11.76	25	Plant sugarcane; eat corn, leaf vegetables; pandanus ripening; fight, but otherwise keep out of rain.
February	wambu-nenai (first-lastborn)	11.35	26	Start pandanus harvest; plant emergency gardens; fight; keep out of rain.
March	iki (single)	12.10	28	Harvest pandanus; corn, beans finished; eat first taro; food short.
April	ni-mupa (gleaning-firstborn)	12.68	24	Eat taro, bush foods; sweet potato scarce; fights over food.
May	ni-nenai (gleaning-lastborn)	7.02	18	Start hunting, cutting house timber, bachelors' rites; food scarce.
June	pindi-mupa (working-firstborn)	5.04	15	Burn off for gardens; trap eels in slack water; hunt; start building houses; sweet potatoes bearing; frosts.
July	pindi-nenai (working-lastborn)	3.70	13	Dig and fence gardens; trap eels; hunt; build houses; prepare for ceremonies; frosts.
August	jambai-mupa (thatching-firstborn)	6.70	19	Dig and fence gardens; start planting; hunt; speed up house-building; make salt; fertility rituals.
September	jambai-nenai (thatching-lastborn)	8.99	23	Finish house-building, gardening, fertility and bachelors' ceremonies; hasten planting corn, taro, potatoes; start trading trips.
October	liu-mupa (plucking-firstborn)	9.11	22	Trading trips; increase funerary and other wealth-distributions.
November	liu-nenai (plucking-lastborn)	11.06	25	Plant sugarcane, bananas; trading trips; wealth-distributions.
December	kumba-mupa (blotted out-firstborn) <sup>6</sup>	11.46	26	The same; fights starting.
	alternative aringgi-mupa (old-firstborn)			

Intercalary month kumba-nenai or aringgi-nenai. Months ni-nenai to liu-mupa are classed as paina- or epe-kana, good months; liu-nenai to ni-mupa are ko-kana, bad months.

when necessary during inter-clan transactions. The list in its main outline holds good throughout the Enga area.<sup>7</sup>

The elders who are calendar-keepers recognize the variation of the ecliptic, which in these mountains is fairly obviously manifested as differences in the position of sunrise through the year. They also realize that, without some adjustment, the lunar calendar would get out of step with the sun's movements and the seasons. To demonstrate how intercalation is managed, I will discuss the matter from the viewpoint of Kara clansmen at Sari.

A range of mountains, rising about 1,000 to 1,500 feet above the Lai River, runs roughly south-south-east from Sari (see fig. 1). Specific clan territories, or localities within these, bear from Sari as marked. Old Kara men have observed that sunrises 'move' from Lupamanda to Taure and back again from the middle of one wet season to the next, that is, from one summer solstice to the next.<sup>8</sup> They

have also observed that the sun appears to rest for some days at Lupamanda before starting its return journey along the range. For reasons which are not now explicit, the lunar sequence begins with the first new moon to appear when the sun starts to move north-north-west along the range. It is also recognized that the sun reaches Taure and moves south-south-east again in the dry season months of *pindi-mupa* and *-nenai*. There is thus a double check on the lunar calendar.

There are other less reliable checks. The ceremonial season, normally the months of *jambai-mupa* and *-nenai* (about August–September) should occur when the sun is moving from Randamanda to Rambam. *Iki* is the wettest period late in the wet season, and should occur as the sun moves from Mukaris to Rambam (about March–April). Slack water in the rivers occurs during the months of *pindi-mupa* and *-nenai*.

Now, let us assume that a given calendrical year begins with the first new moon occurring within a few days of the summer solstice, that is, of the sun's returning from Lupamanda. At the end of the usual 12 lunar months, the thirteenth new moon will appear roughly ten days before the sun moves back a second time from Lupamanda. Because of Lupamanda's bearing from Sari, this discrepancy is not particularly noticeable; but the discrepancy of approximately 20 days at the start of the third year is more obvious. When the twelfth lunar month (*kumba-mupa*) of that year concludes, the sun is only about halfway between Aipinimanda and Lupamanda. So, instead of calling the thirteenth new moon *wambu-mupa*, the old men call it *kumba-nenai*, the second *kumba*. They can then start the new year on the fourteenth new moon, by which time the sun is returning again from Lupamanda.<sup>9</sup> Thus the lunar and solar sequences remain fairly well in step.

Nevertheless, triennial intercalation still leaves a discrepancy between lunar and solar years; three lunar years of 37 synodic months are 3.08 days shorter than the corresponding three solar years. Lunar and solar years

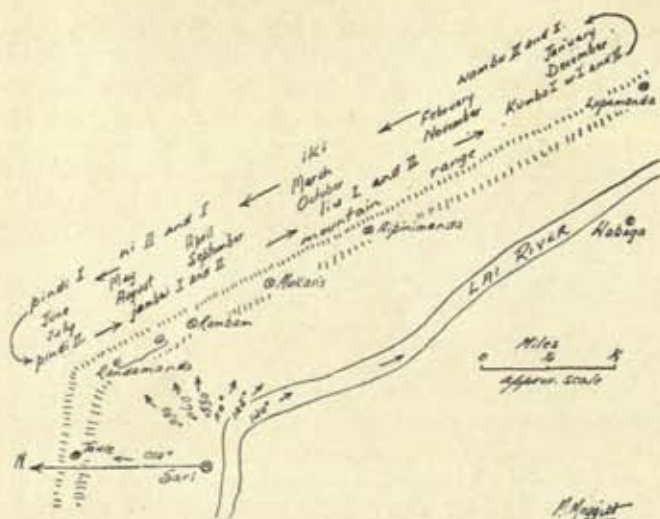


FIG. 1. THE MAE ENGA CALENDAR  
As observed by Kara clansmen at Sari



could be further adjusted by inserting an extra synodic month every 29 years. Although I found no Mae who explicitly stated that adjustment is needed in addition to triennial intercalation, current Mae calendars are generally in step with the variations of the ecliptic. I can only assume that the long-term discrepancy is in fact noted and repaired, although not necessarily in terms of an intercalation every 29 years.

Failure to recognize a 29-year cycle would not be surprising, for Mae rarely calculate accurately periods of years for any reason. There is a term *ju:janai* which means 'the specific place' (*janai*) within 'a greater continuum or space' (*ju:*), and which is equally applicable to events occurring as specific points in on-going activity. But these events are not generally marked in terms of years from a given point. Thus, a man was born 'when the Europeans first arrived,' or 'two wet seasons after the big frost,' or 'before Kara clan defeated Kija clan in the fight over the river gardens.' Younger men, however, can appreciate the European custom of calculating ages in years, and some have worked out rough estimates of their own ages in years. Enga, incidentally, count in fours up to 50,<sup>10</sup> and, in theory, could continue up to 50 units of 50, although I have not observed more than about 200 items counted at any one time.

Similarly, events expected in the future are not simply marked off in terms of a given number of years from the present. Enga support, for example, the *te*, an elaborate exchange cycle analogous to the *moka* at Mount Hagen, which culminates roughly every four years. This interval is largely determined by the breeding rate of pigs. After each large-scale pig-killing, it normally takes four years to breed up pigs mature enough for the next killing. In the same way, fallow ground is brought back into use at appropriate times, not by computing a specific period of years, but by assessing the growth of *Casuarina* and other trees planted on the land when it was previously gardenized. The fallow period calculated in this way tends to allow automatically for

differential soil fertility. In some areas, the fallow period is from four to eight years, in others from ten to 15 years. Where the soil is very poor, especially at higher altitudes, the tree indices are dropped; and an explicit criterion is adopted whereby a worked-out garden should lie fallow until the gardener's sons are adult and ready to use it.

### Notes

<sup>1</sup> Data reported in this paper were collected during fieldwork carried out around Wabag in 1955-57, under the auspices of the Department of Anthropology of the University of Sydney. I thank the University's Research Committee for its financial assistance.

<sup>2</sup> This summary description may be illuminated by referring to W. H. Goodenough, 'Ethnographic Notes on the Mae People . . .', *Southwest. J. Anthropol.*, Vol. IX (1953), pp. 29-44; M. J. Meggitt, 'The Valleys of the Upper Wage and Lai Rivers . . .', *Oceania*, Vol. XXVII (1956), pp. 90-135, and 'Enga Political Organization . . .', *Mankind*, 1957 (forthcoming).

<sup>3</sup> Figures taken from Administration meteorological records kept at Wabag since 1950.

<sup>4</sup> See G. A. M. Bus, 'The Te Festival or Gift Exchange in Enga . . .', *Anthropos*, Vol. XLVI (1951), pp. 813-24; A. P. Elkin, 'Delayed Exchange in Wabag Subdistrict . . .', *Oceania*, Vol. XXIII (1953), pp. 161-201.

<sup>5</sup> See M. J. Meggitt, 'House-Building among the Mae Enga . . .', *Oceania*, Vol. XXVII (1957), pp. 161-76.

<sup>6</sup> That is, south-eastern hills are regularly blotted out by rain coming up the valley in this season.

<sup>7</sup> R. Bulmer (personal communication) reports a similar list among Kyaka Enga of the Baiyer valley to the east, while I recorded another similar list among the Yandapu Enga at Kepilyama to the west. Further west again, however, Ipili people of the Porgera valley have heard of but do not use this calendar. See M. J. Meggitt, 'The Ipili of the Porgera valley . . .', *Oceania*, Vol. XXVIII (1957), pp. 31-35.

<sup>8</sup> In some clan territories, orientation of the mountains necessitates observations of positions of sunsets, or of both sunsets and sunrises.

<sup>9</sup> There is no ritual or other celebration at the new year, or at the occurrence of the solstices.

<sup>10</sup> The unit of four is explicitly related to the four fingers of the hand. Thus, four is *kiru-mendei* (hand-one), eight is *tuku-rapo* (hands-two), ten is *tuku-rapo-erete* (hands-two-completed); then ten named sets of four take the count to 50, i.e. 11-14, 15-18, 19-22, etc.

## SHORTER NOTES

**Tonga Resettlement and the Kariba Dam.** By Dr. P. V.

Tobias, Department of Anatomy, University of the Witwatersrand, Johannesburg

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Not far up the Zambezi River from its crossing by the Great North Road en route from Salisbury to Lusaka, the £114,000,000 Kariba Dam is rising. The damming of the Zambezi will create between Northern and Southern Rhodesia the world's largest man-made lake. As the waters start accumulating in 1959 and 1960, they will inundate some 2,000 square miles of land where today dwell members of the Tonga tribe. In consequence, a vast resettlement scheme is under way, the territorial governments being responsible for moving some 30,000 Africans on the northern side and about 22,000 on the southern side of the river.

Because of this impending move, I was invited by the Rhodes-Livingstone Museum, through its Director, Dr. J. Desmond Clark, to undertake a study of the physical, nutritional and genetic attributes of samples of these Bantu-speaking people. The first season of field work took place in July-August, 1957, and it is hoped to follow it up with a further season in the winter of 1958.

Readers of MAN may be interested in some aspects of the problem, which make this resettlement into a virtually unique experiment in human ecology. The Northern Rhodesian Tonga, amongst whom the first season of field work was carried out, live on the high plateau between Livingstone and Lusaka, and in the low-lying Gwembe Valley, through which the Zambezi pursues its eastward course. The Valley Tonga have become rather sharply distinguished from their brethren on higher ground. On the Plateau, there has been a constant coming and going of Bantu tribes for centuries, and cultural contacts have been free and numerous. As Elizabeth Colson has shown (*Occ. Pap. Rhodes-Livingstone Mus.*, No. 6, 1949), the old tribal ways have metamorphosed under these cultural impacts. The Valley, on the other hand, is forbidding and, until recently, has been most difficult of access. The low-lying river banks down to 1,000 or 1,500 feet, the hot, humid climate, the steep slopes of the scarp, the ravages of crocodile and hippopotamus, of tsetse flies and the trypanosomes of sleeping sickness, of bilharzia and malaria and endemic goitre, of recurrent droughts and sporadic floods, and of primitive, soil-



denuding agricultural techniques, including the absence of irrigation, all conspire to make the Gwembe Valley a most debilitating area. Largely isolated from the tribes of the Plateau, the Valley Tonga have remained relatively untouched by tribal contacts in recent times.

It is from the depths of this Valley that villages are already being moved to higher ground. Each village is being kept intact as an entity on the move, with the result that a long-term follow-up will be possible, not only on population groups, but even on individual villages and persons. In this respect, the Tonga resettlement must be unique among major population movements of modern times. Up to the present, the swift flow of the Zambezi, the fear of crocodiles and the near-absence of fishing techniques and traditions have ruled out fish as a source of nutrition. Officials are wondering whether the resettled Tonga, living instead on the banks of a lake well stocked with edible species, will take to fishing to supplement their diet and help offset the perennial famines which afflict the Valley. Lake-dwelling Africans elsewhere on the continent generally thrive on fish and have well covered bodies compared with those of the Tonga. This is one of the interesting possibilities of the Rhodesian ecological change.

Our field work comprised a number of observations on a series of adults and children, drawn from (i) a control group of Plateau Tonga; (ii) a second control group of Valley Tonga living presently just beyond the future waterline and close to resettlement areas; and (iii) Valley Tonga who are to be resettled. Observations were of a physico-anthropological, genetical, nutritional, medical, and somato-typological character, and close on 200 observations, metrical and non-metrical, were made on each man in the main series, supplemented by photographs of every individual. Among the special features of the survey were the taking of fingerprint records, heights and weights of children, observations on dental eruption, morphology and variations, the incidence and patterns of colour blindness.

From an analysis of these data, it is hoped to be able to draw conclusions about the physical composition, the physique and nutritional status, and the genetical make-up of the Tonga people in the area studied, and especially of those who are to be affected by the resettlement scheme. Such data may, at a later stage, provide a basis for a study on the effects of the resettlement on the physical constitution of the people.

It is hoped, too, that the study may throw up some information on the origins of the Tonga, which are at present unknown. Most of the other Northern Rhodesian Bantu tribes are of Congo origin, having migrated from the great Lunda-Luba Empire of the Congo from the sixteenth century onwards. There is, however, little to connect the Tonga with either the Congo or Tanganyika,

and the evidence—or lack of it—might lead to the inference that the Tonga are one of the oldest Bantu tribes in the territory. The physico-anthropological evidence may well throw light on this problem by uncovering the physical strains which have entered into the racial composition of this tribe. It may be noted provisionally, therefore, that we found—in addition to frankly Negroid and Mediterranean features—a number of faces showing pygmy resemblances, and an important group of faces strongly reminiscent of the big-boned element which Dart has called 'Boskopoid' and Wells 'Bushmanoid.' It may well be that, when the full extent of this prehistoric influence among the Tonga becomes known, we may have to revise some ideas about the early human history and prehistory of Northern Rhodesia. To say more at this stage would be premature.

#### A Museum Exhibit to Outline Primate Evolution. By Dr. E.

H. Ashton, Dr. W. J. Pardoe and Professor Sir S. Zuckerman, C.B., F.R.S., University of Birmingham. With a text figure

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An exhibit has recently been built in the museum of the University of Birmingham Medical School to give a three-dimensional picture of the evolution of the Primates. The display is sub-divided, the first and smaller part comprising five subsections which deal with the evolution of the loris, lemur, tarsier and monkeys of the New and Old Worlds. The second and bigger part is shown in the figure and comprises three sub-sections outlining the evolution of the Asian apes, the African apes and man.

Each subsection comprises a platform 2 feet 6 inches high, on which are displayed extant types; prosimians, monkeys and apes being represented by photographs, skeletons and some stuffed specimens, while photographs and skulls depict living men. A glass case set in front of the platform contains transparent shelves to indicate different geological epochs. Casts and photographs of fossil species are displayed towards the sides of each shelf, while descriptive legends occupy the central areas. From an introductory notice at floor level, arrows painted on the glass case direct a viewer's attention to succeeding legends, and additional arrows point the sequence of the various subsections.

While indicating broad relationships between the major Primate groups, an arrangement of this type avoids any suggestion that direct relationships exist between species from successive geological epochs.

Starting from the notice introducing the smaller part of the exhibit, attention is drawn to the Eocene lemurs and thence to the subsequent evolution of this group. The loris and galago form an offshoot from this subsection, and after examining their develop-

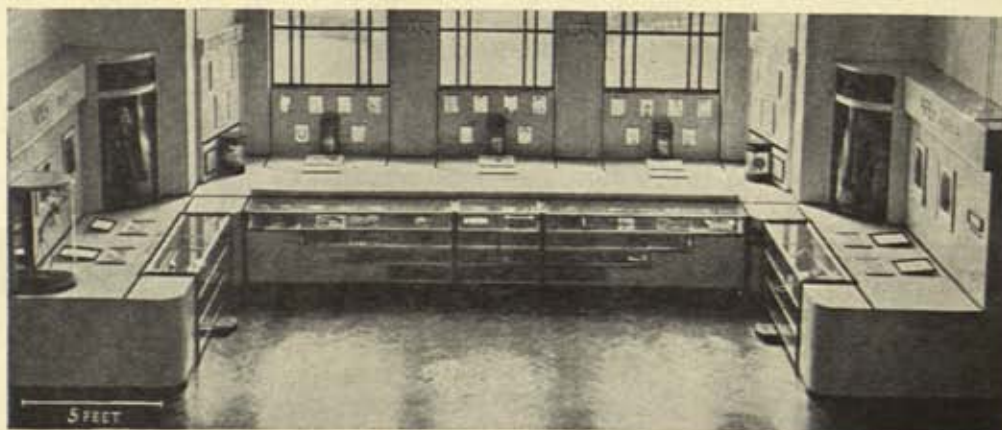


FIG. 1. PART OF THE EXHIBIT DEPICTING THE EVOLUTION OF APES AND MEN



ment, attention is redirected from the introductory notice towards the Eocene tarsiers. From this group, arrows point first towards the living *Tarsius spectrum*, and secondly towards the subsections dealing with the evolution of the New and Old World monkeys, both of which are presumed to have originated from early tarsiers.

The Asian and African apes occupy the sides of the bigger part of the exhibit (fig. 1), an introductory legend mentioning that like the monkeys, apes presumably developed from Eocene tarsiers. Fossil specimens of both lesser and great apes from both continents are represented in the glass cases, and the platform displaying present-day Asian types is divided between the gibbon and orang-utan. Lesser apes died out in Africa before the Pleistocene, and the 'present-day' platform is, on this side, devoted to the chimpanzee and gorilla.

The evolution of man is dealt with in the central subsection of the bigger part of the exhibit (fig. 1), an introductory legend

pointing out that little is known about either the anatomical nature of man's immediate ancestors, or the time at which they separated from the progenitors of the monkeys or the apes. Attention is then drawn to legends describing *Pithecanthropus* and Neandertal man, casts of which are set on the appropriate shelves alongside specimens of the types of stone implements presumed to have been fabricated by these hominids. The remaining parts of the lower shelves of this case depict ancient fossil specimens of *Homo sapiens* and illustrate the types of tools presumed to have been made by him during successive phases of the Pleistocene. The long top shelf illustrates the advance of *Homo sapiens* during the Upper Palaeolithic, and a brief mention of the technological advances during the Neolithic Period leads to the present day. The skulls and photographs illustrating variation in the five extant groups occupy the central part of the platform, while panels set towards the sides give information about the distribution and rate of growth of the world's population.

## CORRESPONDENCE

'A Trobriand Medusa?' A Reply to Dr. Berndt. Cf. MAN, 1954, 158; 1958, 65

90 STR.—I agree with Dr. Berndt that the methodology of symbol-interpretation is an intricate matter but I must assure him that I am very well aware that the Trobrianders make a clear distinction between female witches and male sorcerers and that, since the former are figments of a paranoid imagination, any traditional war magic must have been carried out by sorcerers rather than by witches. I am also aware that among the Trobrianders, as among the English, 'copulation is a fitting subject for verbal abuse.'

All this, however, is entirely irrelevant to the question of whether the Trobriand shield design is, in intention, 'abstract' or 'representational.' My method was to give a visual demonstration that the design might be considered representational and having done so I examined the literature to find out what it might represent. Dr. Berndt proceeds the other way round: he takes it for granted that the design is representational, he looks in the literature for something that (in his view) a war shield might appropriately represent and finally he looks at the design and asserts, quite unconvincingly to my mind, that the expected representation is 'unmistakable.'

And while complaining of Dr. Berndt's methodology I must complain also of his method of quotation. On the very same page from which he manages to extract a sentence to show (as he claims) the unlikelihood of any association between flying witches and warfare Malinowski makes the following remark: 'The disease which witches cause is incurable and extremely rapid in its action, killing as a rule immediately.' Surely a most Medusa-like characteristic? Again, when asserting the importance of the Trobriand taboo on sexual intercourse during times of war, Dr. Berndt alters the sense of what Malinowski actually wrote and omits to mention that Malinowski listed this rule among 'miscellaneous and minor taboos.'

Dr. Berndt's method of interpreting the printed word appears to be similar to his method of interpreting visual design.

E. R. LEACH

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### Notes

The Hon. Editor of MAN was surprised and disappointed that Dr. Leach's very original and suggestive article did not at once lead to a full discussion of this interesting problem, in which recourse might be had not only to the *sortes Malinowskianae* but also to other methods and disciplines. Is there, for example, no follower of Haddon and Balfour left to make a detailed study of as many specimens of these shields as possible and to test or complement Dr. Leach's theory by rigorous morphological analysis? (Malinowski's

own magnificent Trobriand collection is open to the inspection of students in the British Museum.) Dr. Leach, whether he is right or not, has demonstrated a fertile field of interaction which should never have been allowed to lapse under the influence of specialization; and certainly students of material culture ought not, and cannot afford, to ignore actual or potential evidence of this kind, whenever it is available. Further contributions to this correspondence, conceived, it is hoped, in a spirit of fruitful intercourse rather than of battle, will be welcomed.—ED.

'Ulu,' Scraper and Reaping Knife. Cf. MAN, 1958, 38

91 STR.—Prince John Loewenstein illustrates in fig. 3 of his article 'The "Eskimo ulu" in the Malayan Neolithic' iron reaping knives with wooden handles from Indonesia, which are certainly to be connected with similar reaping knives from Kordofan, published by the late Professor Seligman in MAN, 1916, 81, and from Darfur, published by me in *Sudan Notes and Records*, Vol. XX, pp. 308ff., in a note entitled 'The Tigda or Reaping Knife in Darfur.' So we have now another rather inexplicable cultural connexion between Indonesia and Africa. The late Professor Henry Balfour (quoted by me) doubted the soundness of Seligman's comparison of the African reaping knife (which has a straight edge) with a neolithic stone knife with a curved edge, for he thought that the two tools were probably used for different purposes; and certainly what Prince John Loewenstein says about the Eskimo ulu makes it appear to be a descendant of the Upper Palaeolithic scraper. My note has illustrations which show that the reaping knife is used in a very different way from the ulu. It looks too as if some at least of the Malayan neolithic ground and perforated stones from the Raffles Museum, Singapore, may have been used for different purposes still; for instance, example (d) (wrongly marked (e) in Plate D) looks like a stone copy of a leather-cutter's knife, while example (a) looks like some sort of chopper. That the reaping knife has its ancestry in the Neolithic does, however, seem possible from the flint blade with a straight cutting edge set in a piece of stag antler from St. Blaise, Lake Neuchâtel, Switzerland, which is published in the *Cambridge Ancient History*, first volume of Plates, pp. 18f., fig. (f).

A. J. ARKELL

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### Note

The Hon. Editor of MAN much regrets the inadvertence by which Plate D was passed for press with the picture at the foot upside-down, with the result that, as Mr. Arkell points out, (d) was printed at the right and (e) at the left.—ED.



**Extra Dentition among Tibetans.** Cf. MAN, 1957, 227; 1958,

92

44, 45

SIR,—With reference to Mr. Edward Samson's letter in which he invites me to enlarge upon my observation of an extra dentition among Tibetans, I can say that if I made no more than a passing reference to this phenomenon in my article on 'Attrition of the Teeth Among Tibetans' it is because I gave it only fleeting and superficial attention, among the first hundred specimens out of five thousand which I anthropometrically measured in Kalimpong. Gradually, as I progressed with the work over a period of three years, I had begun to notice incidental aspects of Tibetan anatomy upon which I had not been requested to report, and which struck me as worthy of remark. Attrition of the teeth was one of these, and I started to make systematic observations. In the course of the latter, I was also struck by other anomalies such as pearls in molars and extra teeth situated behind the usual ones, especially in the upper maxillary, but I did not note these individual cases with the same care as I did those of attrition upon which I was concentrating at the time. Only later, when working in Copenhagen with Professor P. O. Pedersen, did I discover that these other anomalies were also of great interest, and I therefore quoted from memory when I mentioned them incidentally to the main subject of the article. It may well be that more careful observation would bear out Mr. Samson's point rather than mine, and I can only hope that my remarks will have stimulated sufficient interest for somebody more qualified than I am to go out and investigate.

London

PETER, Prince of Greece and Denmark

**The Art, Myth and Symbolism of Arnhem Land.** Cf. MAN, 1957, 241; 1958, 5, 22

93

SIR,—Mr. Mountford's objections to my review fail to persuade me that I was harsh or one-sided, let alone 'foolish' and 'incapable of understanding the purpose and subject of the book.' His letter confirms that the Expedition consisted of an assemblage of scientists with a number of unrelated objectives in view. One might have expected some evidence of cross-disciplinary fertilization, if not actual co-operation, in this volume, but the opportunity was evidently not seized. No doubt 'goodwill' between the scientists was fostered, but an expedition of this sort seems an elaborate and expensive way of promoting personal friendships.

Mr. Mountford emphasizes that he was merely 'collecting facts.' The most elementary handbook of methodology would tell him that theoretical assumptions influence the very selection and collection of facts, that sense can only be made out of collections of facts in terms of some theoretical scheme, and that theory is not something that can be 'added in' at a later date. My criticism, therefore, is not that there were 'gaps,' but that the facts were collected on the basis of implicit and unanalysed assumptions—for that is all that Mr. Mountford's 'freedom from academic bias' represents.

'Ethnology' of this kind is merely a partial and unsystematic social anthropology. There is, of course, a case for a science of culture, as distinct from social anthropology and sociology (though not a case which I personally accept). This, however, does not seem to be what Mr. Mountford means by 'ethnology.'

Lack of scientific discipline is not to be gloried in but lamented. It

has unfortunate results in this book. How are we to distinguish the 'Just-So' stories from religious myths in this volume? Unless they are 'set in their social context' (Cranstone) we cannot understand them at all. The social context is not an extra frill. The aborigines similarly fail to distinguish between stories like Snow White and stories from *Genesis*.

Nor is our appreciation of the art enriched by this presentation. By what standards is the art to be judged? In terms of 'pure form'? In aboriginal categories? In our own (analysed or unanalysed) terms? How do we (and the aborigines) react to the quite different realistic and abstract art styles shown in this book? This sort of problem and many equally fundamental ones are never even considered in this work. We cannot begin to discuss the 'meaning' of the works until we tackle such questions.

Mr. Mountford's freedom from academic bias unfortunately includes freedom from scientific method and training. I have already pointed to one of the results: the worthlessness of the material presented as 'linguistic texts.' Let me take a further small example: we would have little confidence in the factual accuracy of a foreign visitor who told us that the Queen of England was crowned during a ceremony known as the 'Westminster Abbey.' But Mr. Mountford tells us that the major Groote Eylandt religious ritual is called the *arawalja*. This is a misrendering of *auwarawalja*, the huts or 'shades' erected for the ritual correctly known as the *amunduwuraria*. Many, many facts of this kind are to be found in this volume.

I trust that these observations also go some way to answering the questions raised by Mr. Cranstone in his monthly 'review of reviewers.'

PETER M. WORSLEY

Department of Social Studies, The University, Hull

#### Note

The Hon. Editor of MAN has received a letter from Dr. R. M. Berndt, for which he much regrets that there is no space, supporting Dr. Worsley's review and drawing attention to his own extended and critical review of the same book which is to appear in *Mankind*. The Hon. Editor's attention has further been drawn to a long and generally favourable review by Dr. W. E. H. Stanner in *Meanjin* (University of Melbourne), Spring, 1957, pp. 307-11, and to another by M. Bouteiller in *L'Anthropologie*, Vol. LXI, Nos. 1-2 (1957), pp. 148f.—ED.

**The Study of Race Relations.** Cf. MAN, 1957, 145; 1958, 68

94

SIR,—Having got hold of the wrong end of the stick, Mr. Gutkind proceeds to beat me with it. May I ask him to reread my article and consider whether he has not misunderstood me? He may then recognize that my views are more faithfully reflected in what I wrote than in what he says that I wrote.

Just in case there remains any doubt, allow me to state briefly that in my opinion the study of race relations is the study of the relations between groups when 'racial' notions are brought into play; and that this study must be conducted fully within a general framework of sociological and historical scholarship.

I should like to add that I am sorry that Mr. Gutkind has been puzzled by the Durkheimian formula in the last paragraph of my article.

MAURICE FREEDMAN

London School of Economics and Political Science

## REVIEWS

### AFRICA

**An African Survey.** By Lord Hailey. Revised edn. London (O.U.P.), 1957. Pp. xxvi, 1676. Price £5 5s.

95

This is no ordinary revision. The first edition which was published in 1938 was a demy 8vo volume of 1662 pages excluding the index. The present volume is medium 8vo and contains 1616 pages excluding the index. The amount of new material included in the revision would alone fill a sizable book.

Chapter II, 'The African Peoples,' has been recast and rewritten. The study of blood grouping, which in 1938 had not been carried as

far in Africa as in other parts of the world, had by 1956 advanced considerably. The Survey quotes from Mourant's *Distribution of Human Blood Groups* and concludes that 'though some of the results so far are suggestive, they do not as yet provide material for a complete classification of the indigenous people of Africa. . . . There is also a reference to sickle frequency.'

On the subject of mental characteristics and intelligence tests, Lord Hailey examines the results of recent investigations but remains unconvinced that 'either biology or analytical psychology can



provide any definite evidence of the existence in different ethnic groups of innate characteristics which connote superiority in mind or character as compared with other groups.'

'The need of a wider knowledge of the facts of African life' is as important today as it was when Mary Kingsley first remarked it, and it is good to find sections on 'Studies of African Social Life' and 'Methods of Social Anthropology' including a discussion of the practical value of anthropological studies.

The sections on African Music and African Art are entirely new.

Chapter III, 'The African Languages,' has also been rewritten and the author has made good use of material which has become available since the first edition of the Survey was printed. He discusses the administrative problems created by the multiplicity of languages in the African territories, and the varying degrees of importance which the Administrations attach to a knowledge of the vernacular by their officers in the British, French and Belgian territories.

Remembering the remarkable changes which have occurred since 1938, the reader will not be surprised to find that the chapter on 'Political and Social Objectives' is expanded from 14 pages of demy 8vo to 115 pages of medium 8vo. The concepts underlying the objectives of State policy are examined, and other subheads are 'International Interest in Colonies' and 'The Rising Spirit of Africanism.'

Chapter XXIV, 'The Future of African Studies,' has been rewritten and retitled 'The Organization of Research.'

Thus, as one works through this massive volume one finds everywhere that the revision has not merely brought facts and figures up to date, but has also introduced much original thought on the many and diverse problems discussed.

Owing to its size and weight (nearly 5 lb.) the Survey may come to be treated simply as a book of reference to be consulted only when it is desired to verify a fact or find a figure. If that happens, it will be a pity, for Lord Hailey has put into this new edition even more of his own penetrating analysis, and so much of his wisdom, and this all needs to be read now.

One criticism is that the valuable Chapter XXV, 'Summary and Conclusions,' has disappeared. One presumes that it was crowded out.

G. BERESFORD-STOOKE

#### **Ethnographic Survey of Africa: Pagan Peoples of the Central Area of Northern Nigeria.** By H. D. Gunn. London (Internat. Afr. Inst.), 1956. Pp. 144. Price 17s. 6d.

96

The area covered by this volume of the Ethnographic Survey forms the north-west arc of the borders of the Jos Plateau in Northern Nigeria, but has no special geographical or ethnographical unity. More than 50 small tribes inhabiting the area are described in this report as comprehensively as the sources allow, and they are provisionally classified in six larger groupings. Most of the information about these peoples, apart from Meek's work, is contained in unpublished administrative records, and the relevant ethnographical material has been extracted down to the last ounce by this survey. The rarity of this material justifies the author's decision to publish it in detail, but the amount of general information provided about the background of the tribes is not enough to lighten the narrative and prevent it from becoming stodgy in places. There are few references to other pagan groups in Northern Nigeria, and it is not made clear whether the six main groups would merge in relation to a wider cultural survey or whether they are to be regarded as absolute divisions. In the historical section, similarly, local details are emphasized at the expense of background information, and there is no reference to the archaeological finds which provide evidence of a former unity of culture in the area.

J. S. BOSTON

#### **Ethnographic Survey of Africa: Western Africa, Part XI, Coastal Bantu of the Cameroons.** By Edwin Ardener. London (Internat. Afr. Inst.), 1956. Pp. 114. Price 16s.

97

This volume of the *Ethnographic Survey* concerns a number of numerically small peoples classified in three sub-groups as the Kpe-Mboko, Duala-Limba and Tanga-Yasa. All live within the immediate coastal area of the French and British Cameroons. Of

these peoples the two largest, the Duala and Kpe (or 'Bakweri'), number 23,000 and 15,600 respectively, the remainder numbering only a few thousand each, with some less than a thousand. Earlier literature is extremely sparse. Valuable use is made of this, but the volume relies mainly upon the author's own fieldwork in this area, which included an intensive study of the Kpe and associated enquiries amongst the Mboko, Wovea, Isuwu and Kole. In the arrangement of the volume the various peoples or sub-groups are described consecutively under the main ethnographical headings rather than given separate, self-contained sections; this makes for difficult reading, but it provides the opportunity for much cross-reference, where, with so little information concerning the other peoples, the Kpe form the basis of the description and emerge most clearly from the composite picture.

The Kpe live in a series of small villages on the slopes of the Cameroon Mountain, where they neighbour (and in some cases are surrounded by) the extensive plantations originally developed by the Germans but now run by the Cameroons Development Corporation. So situated, they have been subjected to considerable European influence, even 'upheaval.' Unlike the Duala, however, who formed one of the important West Coast trading communities of the nineteenth century, the Kpe have not taken easily to European development; Mr. Ardener writes of their 'reserved' attitude and of 'what is known as "Bakweri apathy".' Both Kpe and Duala have a political organization based on their 'village' or 'town' settlements. Within this organization and in part associated with it there exists a system of named patrilineages: in the more concentrated settlements of the Duala these may occupy distinct areas—wards or quarters—of the town; amongst the Kpe members of the patrilineage tend to be dispersed within the village. In the kinship context, however, there is some stress on both patrilineal and matrilineal lines of descent; the Kpe kinship system is described as one of explicit double descent. Closed associations or 'secret societies' figure in both groups, but amongst the Kpe at least would appear to be more especially dance associations, with some supernatural powers attributed to their members, than law-enforcing bodies of the Cross River 'Egbo' or Ngbe type. In the past, acquisition of status by wealth was finalized for the Kpe with the performance of a ceremony (*ngbaya*) in which livestock were killed and distributed, this ceremony also having ritual aspects. A complicated series of mortuary rites are also recorded, including a ceremony similar to *ngbaya*, performed for an important man, in which large numbers of livestock were killed.

The volume is carefully written; it contains much detailed and concise information, and is a useful addition to our knowledge of this ethnographically difficult and little explored region.

M. J. RUEL

#### **Ethnographic Survey of Africa: The Ila-Tonga Peoples of North-Western Rhodesia.** By M. A. Jaspán. Edited by Daryll Forde. London (Internat. Afr. Inst.), 1953. Pp. 72, map. Price 8s. 6d.

98

The writer of this book has had no easy task since, other than Smith and Dale on the Ila and Colson on the Plateau Tonga, little of value exists concerning the associated tribes in Northern Rhodesia which are generally referred to as the Ila-Tonga people. This account naturally follows, therefore, the writings of the above-mentioned authors and as not a little change has taken place among the Ila since the authoritative work was published just after the First World War, the section on this tribe needs bringing up to date in some matters. It is unfortunate that greater use could not have been made of official records, both published and unpublished, to assist in this. The present work does however enable a comparison to be made between Tonga and Ila, though one would have wished that the author had devoted some space to comparison in the text. There is also a useful bibliography which will form a basis for further study.

The very meagre historical background concerning the origin of these tribes will only be supplemented by archaeological research since oral tradition is absent for all but the last century. There is no doubt that these people were some of the earliest of the existing Northern Rhodesian peoples to establish themselves in the territory, a



circumstance which must have taken place some time before the sixteenth century. That they are now among the most advanced in European farming methods of any tribe in the territory says much for their adaptability to changing economic conditions.

Some minor criticisms, not due to the author's fault but deriving from the quantity of inaccuracies in previous literature, could be made. The author deals adequately, to the limit of the material at his disposal, with the tradition, economy, social and political organization and religious beliefs of the Ila and Plateau Tonga, the general pattern of whose life is closely reproduced among the other tribal groups usually associated with them.

J. DESMOND CLARK

**Ethnographic Survey of Africa: Peoples of South-West Ethiopia and its Borderland.** By Ernesta Cerulli. London (Internat. Afr. Inst.), 1956. Pp. 148. Price 17s. 6d.

99

This section deals with such of the above-mentioned peoples, and of North-west Ethiopia, as have not been classified as Galla, Nilotes, etc., and dealt with in earlier sections of the Survey. The industry of the author is attested by a bibliography of some 250 items, and she has included for what they are worth, which obviously is often very little, all references to these little-known peoples. Her method of presentation has, however, made what was bound to be an ethnographical scrapbook scrappier than it need have been. The tribes are collected into groups, and each item in the culture of a tribe must be looked for under a group heading. The Gamo of the Omo Basin, for example, are grouped with 20 other tribes. Under 'Grouping, Nomenclature and Location' they receive 11 lines on p. 96. On p. 101, under 'Hunting and Fishing,' we read: 'Gamo; it was customary to emasculate an enemy when he had been killed.' Under the heading of 'Social Organization and Political System' all we are told of the Gamo is: 'Gamo; a divorced wife may take with her only the clothes she is wearing' (p. 103). We turn hopefully to 'Dress and Ornaments,' but there, alas, we find no mention of the Gamo. It would surely be better to collect what is recorded of each tribe in one place.

The Lopit Hills, in the extreme south-east, are the only part of the area with which I am acquainted. They are mentioned on pp. 69 and 71 four times as 'Lopit' and four times as 'Lafit.' Only the former is indexed. 'Durrha' is mentioned many times, but we are not told what it is. It can hardly be meant for *dhura*, millet, for it is mentioned with millet on p. 43.

One fact which emerges from the volume is that Ethiopia is probably the least-known country in the world. RAGLAN

**Government and Politics in Tribal Societies.** By I. Schapera. London (Watts), 1956. Pp. 238. Price £1 1s.

100

This book is as important for the political scientist as for the social anthropologist. It is an analysis of the political and administrative process in societies at an early stage of development. More precisely, the subjects of this study are four groups of people in Southern Africa, two of which (the Bushmen and the Bergdama) remain at a primitive stage of advancement with scattered population, primitive agricultural techniques, and two (the Hottentots and the Bantu) are already comparable in internal structure to western societies.

The value of this book for a political scientist is that it is an analysis of societies where the wood can be seen for the trees. Normally, the student of administration is overwhelmed by the apparent diversity and purely contingent problems of the objects of his study. In Professor Schapera's study it is possible to see the basic, primordial structure of society, and to distinguish the essentials from the inessentials.

There are three sets of questions which interest the political scientist. If he is primarily concerned with the legal origins of the state he attempts to distinguish the basic elements which will qualify a community as 'a state.' Secondly, he may be mainly concerned with the political processes within a society and attempt to detect those elements which act as the dynamic in human intercourse, which give impulse and direction to society's activities.

Thirdly, he may be primarily concerned with the administration of society, the mechanics whereby society regulates its public affairs, maintains stability, and protects the *status quo*.

He will have a great deal to learn from Professor Schapera in all these fields. This book goes beyond the normal sociological treatise, and beyond the special studies in social anthropology to which we are accustomed, and poses the questions with which political scientists, too, are concerned. This is a book which talks the same language as we do and is concerned with the same problems as we are. It is a most successful attempt to straddle different disciplines.

Professor Schapera's first important point is to test and reject the theory that political aggregations in primitive society are necessarily based on kinship and family (from evidence obtained from the four tribes whom he studies). Several studies had already indicated that this was by no means invariably true, but the theory continued to be given some credence. The evidence presented here should demolish it definitively. The point is important for a political scientist. If primitive societies are based entirely on kinship and family their internal arrangements will inevitably be of quite a different kind from those found in any form of modern society. If, however, membership of a primitive community can be obtained by methods familiar to us—by marriage, conquest, immigration, adoption, flight and so on—the internal structure of primitive communities will reflect some of the stresses and operations common to a mixed society, and examination of its internal politics and government will have some relevance to the study of the origins of modern administration.

Professor Schapera's four peoples are sufficiently different to give a broad range of early human experience in the field of politics and administration. He examines in detail the framework of government, the activities of tribal governments, the privileges and powers of office, the relations between rulers and subjects, and the forms of tribal government. All these topics are of the greatest importance to the political scientist. We can trace the emergence of the government process as Professor Schapera takes us from his most primitive to his most advanced examples. Certain constants are to be found. The degree to which government is personalized and the lack of bureaucratic apparatus are striking. The evidence for the growth of a class structure is most interesting, as is the demonstration that (with one doubtful exception) government has not followed in the track of conquest, chieftainship seeming to be, as the author remarks, a traditional feature of all these peoples whether or not they expanded by subjugating other groups. Indeed, the development of a class structure to the point where one section of the population is systematically exploited seems to arise only when conquered and conquerors have markedly different linguistic and cultural characteristics.

All this has familiar echoes for the political scientist, with one vital exception. If one studies the history of administration in Europe one notes that the constituent elements of administration common to (say) the Roman Empire, fourteenth-century Castile and nineteenth-century France, are justice, defence, finance, foreign affairs and internal affairs (law and order). If these constituent elements are traced further back in individual countries we are tempted to regard finance and foreign affairs as incidentals, contingent factors. Finance seems to become simply part of household management, and foreign affairs, in so far as they existed, tended to be absorbed by defence. We have tended therefore to expect justice, internal affairs, and defence to be the basic primordial aspects of administration. This emphasis on those features of government involving the exercise of, or the right to exercise, physical force can be shown in many writers, whether lawyers, social anthropologists or political scientists.

But Professor Schapera shows, convincingly I think, that the enforcement of law and order and the physical aspects of compulsion are not necessarily one of the primordial features of government. He shows that the Bergdama and the Bushmen can manage their public affairs adequately despite their lack of force, and despite the inability of their chiefs to punish offenders in other ways. He suggests that instead of justice, defence, law and order being the first elements of government, precedence should be given to foreign affairs (independence of external control), and co-operation in domestic affairs



or what we should now regard as finance or household management function.

If this primacy is accepted several puzzling features in the history of public administration disappear. Development is far more logical. Distinct social classes appear as the population becomes diversified; the provision of public services spreads from the simple direction of co-operative enterprises to the institution of private rights in land, to a rudimentary taxation system and to systematic administration of justice. A special political staff attached to the ruler emerges, and governmental power is transmitted to regional and local rulers; the ruler himself is gradually elevated to a pre-eminent position with extensive powers over property and people. Finally, we arrive at a world similar to our own, with placemen, pressure groups, civil and foreign wars and special techniques of control.

This is a fascinating story, always supported by extensive practical instances and wide learning.

I am puzzled by one thing. Throughout this book Professor Schapera assists his readers in making mental comparisons with other societies by calling attention to significant aspects of his argument. But for some reason he always calls attention to the differences between his examples drawn from these four southern African peoples and their counterparts in modern western societies. In fact, it would always be much more significant to draw attention to their similarities with early and medieval administration. For instance, he discusses the lack of formally separating legislative, executive and judicial bodies in his examples. But this is not illuminating; the idea of such a distinction is a purely eighteenth-century idea, and there are few political scientists today who would like to define 'executive' or 'judicial' in a modern state. But there is surely close comparison to be made with early European government where all these functions were in some way intimately connected with the King's household. The growth of councils may also be illustrated from medieval sources, and with much the same features and origins as those described here, while there are significant echoes of 'the King's peace' in the description given here of justice amongst the Cape Nguni.

Fortunately the fact that Professor Schapera is not comparing like with like has no methodological consequences; it is a minor irritant to the over-scrupulous. It in no way effects the lasting value of this absorbing book.

BRIAN CHAPMAN

**African Leopard Men.** By Birger Lindskog. *Studia Ethnographica Upsaliensia*, Vol. VII. Upsala (Almqvist & Wiksells), 1954. Pp. xii, 219. Price £3

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The author of this flamboyantly produced book concerns himself with that phenomenon dearest of all to the writer of African adventure stories—the Leopard Man: it is therefore only fair to warn the prospective reader that the sinister iron claw which gleams from the dust-cover presages not ten chapters of breathless, jungly suspense but a meticulous and scholarly 'debunking' investigation.

A survey based upon anthropological monographs, court and missionary records, newspaper articles, government reports and a variety of other African sources reveals that about the middle of the nineteenth century there started a series of murders said to have been conducted for ritual purposes by men disguised as leopards who belonged to powerful secret societies. Outbreaks of killing attributed to the leopard men have recurred sporadically ever since this period, in a transverse belt stretching from Sierra Leone in the west, through coastal Guinea and the Congo, to Tanganyika, Mozambique and the Rhodesias in the east; and although widely varying interpretations have been put upon these events, the author shows that descriptions of the Leopard Man and his grisly deeds are sufficiently uniform throughout their distribution range to be reducible to a standard scheme.

In strong contrast with this uniformity is a surprising inadequacy of actual evidence. A review of legal material shows that the ground for conviction was in most cases either unreliable testimony or else the possession of instruments which were never proved to have had an unequivocal connexion with murder. Other data are equally

tenuous. Faced with this discrepancy between the tradition and the evidence, the author concludes that the Leopard Man and his secret society must be viewed as mythical entities, born from the European misinterpretation of widely current African beliefs and metaphors, nourished by a selection and overweighting of evidence to which both sides were strongly motivated, and propagated by diffusion from region to region wherever there was a repetition of the social conditions that first aided their growth.

Dr. Lindskog, as I am sure he would be the first to admit, has not disproved the existence either of Leopard Men or of Leopard Societies—indeed, the *ex hypothesi* aura of secrecy which shrouds their activities makes such disproof almost impossible—but he has nevertheless shown in a penetrating analysis of the records that on currently available evidence neither the Man nor the Society can be accepted as fact.

As a study in the genesis of a myth which was adopted almost as readily by administrators as by their wards, and as an object lesson in the misunderstandings to which language differences can give rise, this is a book which will provide both salutary reading for colonial officials and absorbing material for theorists in the sociology of belief. Its great interest gives one all the more cause to lament a very ponderous style which, especially in the two concluding chapters, does a good deal to obscure the quality of a fascinating piece of research. This defect, however, may well be the fault not so much of the author as of his translators. W. R. G. HORTON

**Tiv Farm and Settlement.** By P. J. Bohannan. London (H.M.S.O.), 1954. Pp. 84. Price £1 7s. 6d.

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This monograph by the joint author of *The Tiv of Central Nigeria* (1953) is No. 15 in the series of Colonial Research Studies. It was in the course of doing fieldwork for their book that Paul and Laura Bohannan became interested in Tiv farms and Tiv domestic arrangements. In Paul Bohannan's own words: 'The farm and compound were the standards of relevance for selecting data; they provide as well the focusing point for presentation of the data. Concern is with land rights and land disputes, with boundaries and acreages, with the nature of labour groups, consumption groups and two different sorts of co-residential groups; we are further concerned with crops and crop rotations, with the agricultural calendar, and with the size of Tiv farms.'

In any specific parcel of cultivated land, there are three clusters of rights: those of the compound head to allocate and re-allocate land among members of his compound; those of the male family head to be given land for his family's needs; and those of every male family head's wife or widow who combines the control of most of the produce of the land with a corresponding obligation to feed her children and husband thereout. But one vital condition of all three types of rights is that the quantum of land so allocated must be adequate to the specific needs of the grantee; this necessarily implies that failure to cultivate an allotment effectively may work a forfeiture. The compound head replaces the chief in Yoruba or Hausa society, since the Tiv, like the normal Ibo, society has no traditional chief.

Bohannan also examines the issue of individual *versus* communal ownership, and is quite emphatic that 'among Tiv, rights to land are individualistic,' but also that 'the compound is the largest unit which has rights in land.' Again Tiv rules of inheritance are that when the fields are 'split' a man has a right to that portion of his father's land which was farmed by his mother, and that full brothers split their land only on the basis of what their respective wives have farmed. In legal parlance this is division *per stirpes* according to the number of wives in the land-owning household. All these seem very similar to what obtains among other peoples of Nigeria like the Yoruba and the Ibo. So also seem Tiv notions about non-alienation, temporary transfer of land rights, periods of fallow, playing host to stranger communities and individuals in land allocation, slave rights in land and of inheritance, and so on.

All in all, this is a carefully detailed study of Tiv land tenure, and is well illustrated by helpful tables, diagrams, maps and photographs.

T. O. ELIAS



**Efik Traders of Old Calabar.** Edited by Daryll Forde. London (O.U.P.), 1956. Pp. xiv, 166. Price 16s.

**103** Antera Duke was a leading trader in eighteenth-century Calabar. He had well equipped canoes which plied the Cross and Calabar rivers linking the coastal towns with the interior. In the hinterland these canoes obtained slaves and produce which were exchanged for European manufactured goods on the coast. Antera also fully participated in the life of the society to which he belonged. He was a senior member of the famous Egbo society, 'a graded association of freemen which controlled and was controlled by the wealthy leaders of the Efik towns.' He freely entertained and was sumptuously entertained by both his kith and kin and the European captains and supercargoes who regularly visited Calabar. Through these European traders Antera and others of his class recognized the advantage of the written over the unwritten record, and long before the arrival of Christian missionaries in Calabar in the mid-nineteenth century many Efik traders had begun to keep records and diaries.

*Efik Traders of Old Calabar* is a symposium centred on what is extant of the diary of Antera Duke, covering the period 18 January, 1785, to 31 January, 1788. Its purpose is twofold: 'to publish an important document concerning the history of Old Calabar' and 'to provide an ethnographic background and a sociological interpretation for this and other early writings on the Efik' (p. ix). This purpose has to a large extent been achieved.

The diary itself is a curious document. The original text, written in Antera's hand, was in Pidgin—the language employed in the commercial and social dealings of the Efik with overseas merchants, a language with a largely English vocabulary modelled on the local idiom. And the diary came to see the light of day in a curious manner recounted by Professor Daryll Forde in the introduction. The 'translation' of the text into modern English—the combined effort of the Rev. Dr. A. W. Wilkie, the saviour of the diary, and Mr. D. Simmons, its annotator—has greatly widened the circle to which the document will prove interesting. Through these salvaged fragments, for instance, one can catch glimpses of how 'palavers' (disputes) were settled in Calabar, how the 'trust trade' was carried on, the compounds and private establishments of the chiefs, the eating and drinking and merrymaking of the people on occasions, the illness, death and funeral of Duke Ephraim, and other useful information which may interest the historian or sociologist as well as the general reader.

Mr. Simmons's annotation of the diary is concise, lucid, instructive. He not only supplies the background necessary for a clear understanding of the text, but goes down to the root and origin of words to show how they have worked their way into the Pidgin of the West Coast. But there is more to some practices of the coastal traders than Mr. Simmons has shown in his sketch. Take the question of 'dashes' or 'presents': to his quotation (p. 5) of J. Smith's catalogue of things for which Africans received a 'dash' he only prefixes the remark that this was 'an important aspect of trading.' The word 'dash,' however, embraced what the European traders would call pilotage, wharf fees, agents' commission, subsidy, and so on.

Mr. G. I. Jones has carried out a careful study of Efik society which is as penetrating as it is intelligent, the result of years of fruitful residence and patient inquiry among the Efik. The cornerstone of this society was the Egbo confraternity, a secret organization whose privileges and functions are indicative of other means than monarchical rule whereby Nigerians of the South maintained law and order before the British advent. In Mr. Jones's survey one would have liked to know more about 'the tendency for conspicuous waste' to which he partly attributes 'the increasing number of victims' in the funeral obsequies of the big men of Calabar (p. 153). It would also seem from his analysis that religious considerations were almost absent from Efik thought of the period. The wonder is not that there later developed these 'outbreaks of mutual destruction,' which form the predominant note in Mr. Jones's final section, but that, in spite of natural jealousies and tensions among the Efik, and in contrast to events in the neighbouring Oil Rivers, Egbo was successful in fostering the conditions necessary for the peaceful development of the palm-oil trade in the nineteenth century.

Professor Forde's editorship bears the definite impress of a knowledgeable co-ordinator, and the success which he has achieved in this work is all the more enhanced by the independence enjoyed by the contributors.

*Efik Traders of Old Calabar* is in line with recent developments in the investigation of Nigerian records and the writing of Nigerian history. Until quite recently writers had paid scant attention to records kept by Nigerians themselves or to the oral tradition of the people. But this attitude is changing. And just as the gradual disclosure of Arabic records in Northern Nigeria, for instance, is leading to a review of some historical assumptions about that region, so the dissemination of information like that in *Efik Traders* will lead to a more and more accurate appreciation of the 'Atlantic community' of the Nigerian coast.

C. CHIEKA IFEMESIA

**Shaka Zulu: The Rise of the Zulu Empire.** By E. A. Ritter.

Fourth impression. London (Longmans Green), 1956. Pp. 383. Price £1 5s.

**104** The phenomenal rise of the Zulu under their ruler Shaka is one of the few well documented events in African history. We have several accounts by Europeans who were eyewitnesses, and there are also a large number of Zulu accounts. It has been described in recent years by Bryant in his *Olden Times in Zululand and Natal*, and also by Mofolo in his *Shaka: An Historical Romance*. *Shaka Zulu* is a more popular piece of writing, in the form of a semi-fictional biography. It is hardly history, and we are given almost nothing of the social and historical background to these few years of sudden conquest and bloodshed.

The account Mr. Ritter gives us is exciting enough, but is marred by romanticism, sentimentality and a pedestrian style that occasionally descends to mere clumsiness or an embarrassing use of 'poetic' imagery of a fanciful hue. His story is based upon those of Bryant and Fynn, with a good deal of oral Zulu material. We get plenty of layman's anthropology, taken mostly from Krige. Some of Shaka's battles are analysed in detail and make most interesting reading. There is also much *verbatim* conversation, permissible in a book of this sort but all rather stodgy: we never really discover what sort of person Shaka was. Mr. Ritter is concerned to show that many of the despotic actions popularly attributed to him never occurred in fact, and that he was a kindlier man than some accounts would have us believe. But the author's attempts to describe Shaka's psychological development are not very convincing.

The entry of Europeans into the story is told rather vaguely, summed up in the statement that 'one outstanding fact, however, emerges and stands forth like a shining beacon above the haze of time and controversy, and that is that the White men had some dominant quality even when in rags which compelled the black men to regard them as their superior.' This was *ubu-kozi*, a single word 'which fully defines that otherwise indefinable aristocratic tendency which radiates authority without any apparent effort' (p. 268). This expresses a general tone that tends to pervade the whole book. The Zulu are shown as truly noble savages, although given occasionally to considerable cruelty, as indeed are most noble savages; there is hardly the mention, and perhaps hardly the realization, that they still exist today. But fair is fair: Mr. Ritter has set out to write a popular account of a period eventful in African history, and it is well worth reading.

JOHN MIDDLETON

**Ngoma Lungundu: Eine afrikanische Bundeslade.** By Harald von Sicard. Stud. Ethnog. Upsal., V. Uppsala (U.K. Agents: Kegan Paul), 1952. Pp. viii, 192, illus., bibliog. Price £3

**105** The object of this study is a Venda myth from the region of Zoutpansberg in northern Transvaal, South Africa. The story, called 'Ngoma Lungundu,' was recorded by E. Mudau (a member of the Venda tribe) for the annual literary competition held by the International Institute of African Languages and Cultures. It was N. J. Van Warmelo, government ethnologist of the Union of South Africa, who, in his *The Copper Miners of Musina and the Early History of the Zoutpansberg*, Pretoria, 1940, drew special attention



to this narrative, because he was struck by some remarkable parallels between the Venda traditions recorded by Mudau and certain aspects of the early history of Israel, as described in the Old Testament. It is this striking parallel between Ngoma Lungundu—the Drum of Mwali (Mwari), the Ancestor God of the Vhasenzi (Vhavenda)—and the Ark of the Covenant of Jehovah, in the Old Testament, which highly puzzles the author, and in this book he tries to find the solution for the problem.

Bible quotations, carefully selected to suit his argument, are compared with quotations from the Venda story. One chapter is dedicated to Mwari, and here resemblances are sought and found with Jehovah. The Vhalemba, who in the story are privileged to carry the Holy Drum, are compared with the Levites of the Old Testament. In the last chapter the Ngoma Lungundu story is compared to *Kebra Nagast*, the Holy Book of the Ethiopians. The author's conclusion is that the Ngoma Lungundu tradition has travelled southwards via Ethiopia, and has become mingled with Lemba elements of Falasha origin from Ethiopia.

After reading the book, one wonders what has been the aim of the author in this study. Many questions arise for which no answer is given. The relationship between the Vhavenda, Vhalemba and Ethiopian tribes, mentioned in the book, is not made clear. A systematic ethnological approach is lacking. The author's far-reaching conclusions based on the arguments are not convincing enough. Through lack of system as well as of factual data, this study loses value for the anthropologist.

R. S. WASSING

**Bantu Bureaucracy: A Study of Integration and Conflict in the Political Institutions of an East African People.** By Lloyd A. Fallers. Cambridge (Heffer for East Afr. Inst. of Soc. Res.), 1956. Pp. xiv, 283. Price £1 10s.

Sixty years ago the Soga were subsistence cultivators with a few cattle and some small stock, organized within a system of small kingdom-states, often enough at war with each other and with neighbouring peoples. Today they form a unified political group of peasant cultivators, growing cotton for cash, and integrated administratively into the Uganda Protectorate. Further changes are imminent, and the future outlines of Soga society already perceptible. This process of rapid change, particularly as it affects political institutions, is the main subject of Dr. Fallers's book. He presents his material as an essay in applied anthropology, for he sets out not only to provide a case study for wider comparative analysis, but also to provide data for those who must take the critical decisions regarding Busoga's future state.

His main theoretical preoccupation is with 'the dimension of conflict and integration among particular institutions, and . . . the consequences of the operation of such institutions for persistence and change in society as a whole' (p. 226). His method is to analyse Soga political institutions at two points in time: first, immediately before the British arrived, and second, at the present day. The analysis of traditional Soga society is based on the interesting contention that as authority then was structured in terms both of corporate lineages and a state organization whose principles were in conflict, this produced 'strain' and 'instability.' The fundamental tendency towards sibling equivalence brings lineage organization into conflict with state hierarchy. Because of the threatened danger from other royal princes, the ruler protects himself by appointing commoner client-chiefs, whose loyalties are entirely to his own person, as administrative officers of the state. In turn, these commoners could exercise authority within their own lineages through these official positions.

I do not feel that the argument is clinched by the description of the traditional system. The important question of the former method of succession is obscure, for the present division between the kinship successor and the heir to land and other property is said to be a 'recent pattern' (p. 91). There are also indications that not all royal princes had equal claims to be considered for succession, and that 'sons of recent rulers' had a superior claim (p. 134). Although it is clear that certain administrative officials, particularly war leaders, owed their elevation entirely to the ruler, there was a tendency for their position to 'become unofficially hereditary' (p. 140), and some of the other commoner chiefs held positions in the state hierarchy

which were apparently hereditary. Representatives of several 'large and important clans' had special duties during the installation of the royal ruler, some of whom were also 'guardians' of royal insignia (p. 133). It seems possible that these duties were hereditary. The role of this group of commoners would appear to be political as well as ritual, for presumably they could exercise influence on the choice of a ruler by withholding their services. Other important commoner chiefs united with the royal lineage to select the new ruler, and these 'often had the deciding voice' (p. 135). A precise description of the kinship relationships between the members of such a council of commoner chiefs and royal princes in one kingdom would be invaluable for understanding the traditional Soga system, but this kind of information is now beyond recall, for the British administration has replaced the old chiefs and rulers and councils with a local government organized on bureaucratic principles.

Dr. Fallers examines the different stages of this transformation and analyses the composition of the present civil service, how its members are recruited, the ties of kinship between them and their effects, and its relationships with both the British administrators above and the peasants below. The conflicts inherent in the position of civil-service chief are discussed with great clarity and insight. The hereditary principle has been abandoned in recruiting civil-service chiefs, but remains paramount in the selection of headmen. The position of the village headman in this system of government is of the greatest interest, for he is at the same time the lowest official in the bureaucratic hierarchy, although unpaid (at the request of the headmen themselves), and the leader of his local group, often in opposition to the hierarchy. The main source of his power today is his responsibility for allocating land by traditional methods, for as yet there is no freehold or any method of buying and selling land. Land has acquired a new economic value since the development of cash cropping, and is the main source of cash revenue for Soga peasants, few of whom leave their country to work abroad. The headman is strongly bound to his villagers by kinship ties, and, as Dr. Fallers notes, the system of corporate lineages has firm roots in the system of land-holding. The headmen often regard the Government as 'hostile,' and tend to believe that 'the Government was trying to steal their land' (p. 177). Future economic and social developments in Uganda, which are likely to entail more individual forms of land-holding and the use of modern agricultural techniques, are bound to come into conflict with this fundamental attitude. This common problem, found in similar forms in other parts of Africa, does not lend itself to any easy solution; Dr. Fallers has placed it firmly in its social context among the Soga.

This admirable book deserves to be widely discussed, both for its approach and for its subject matter. Dr. Fallers considers that both sociologists and social anthropologists are now concerned with the same problems, and would use the terms interchangeably, a proposition unlikely to commend itself to everyone, and he tackles a delicate administrative field which is usually the subject of heated opinion rather than detached analysis. He has made a lively contribution to African studies.

W. WATSON

**The Iteso: Fifty Years of Change in a Nilo-Hamitic Tribe of Uganda.** By J. C. D. Lawrance, with a Foreword by Sir Andrew Cohen. London (O.U.P.), 1957. Pp. xx, 280, 10 plates, 22 text figs., map. Price £1 16s.

Our knowledge of the Nilo-Hamitic tribes of north-east Uganda (apart from that supplied by Gulliver's recent book *The Family Herds on the Jie*), and of the Teso in particular, is not extensive, and it is therefore all the more to be regretted that this lavishly produced book of 280 pages does not add more effectively to that knowledge. It does, of course, add something; but it reads rather like an unusually full and carefully compiled District Book (the kind that is seldom met with) which would be of great use to administrative officers, but is of much less use to anthropologists and others. The best parts of the book are Part I, which deals with the history of the tribe, and Part IV, which is a useful and well illustrated contribution to the material culture of the Teso, a subject on which we had previously hardly any information. The treatment of social organization, political structure and religion is sketchy, though there is a painstaking account of the age-set system, which was suppressed in 1900,



so that the account is in the nature of a reconstruction. In fairness to the author, it must be mentioned that, as he himself says, the whole pattern of Teso culture was radically altered by the subjugation of the tribe by the Ganda general Kakunguru in 1899, with the result that not only the age-set system but other aspects of tribal life must be reconstructed if any record of them is to be preserved. The Teso still have a social life, however, and this should have been described in greater detail; even the Ganda conquest cannot have left them with, for example, nothing in this field but a list of relationship terms; and in this connexion, and elsewhere in the book, we are left with the feeling that we have not really begun to know the people. It will not do for an author today to disclaim any anthropological knowledge (p. vii); the modern reader expects something better than books like Roscoe's *Bagesu*. However, this book does to some extent help to fill the gap in our knowledge of the Teso, and for what we are given we are grateful.

G. W. B. HUNTINGFORD

**Pescatori dell' Oceano Indiano: Saggio Etnologico Preliminare sui Bagijuni, Bantu Costieri dell' Oltregiuba.** By Vinigi L. Grottanelli. Rome (Edizioni Cremonese), 1955. Pp. xxi, 409, 109 text figs., map.

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Price 3,500 lire

This is the first detailed study of one of the peoples of the East African coast—the Bajuni (also known as Wagunya and Watikuu), a Bantu(Swahili)-speaking people of mainly non-African stock. In addition to the ethnographical account of the Bajuni, Professor Grottanelli has included two valuable studies of the historical and archaeological aspects of the Asiatic coast settlements (chaps. 1 and 8).

The book deals at length with the economics and material culture of the Bajuni (chaps. 3 and 4), and in less detail with their political institutions, social organization, and religion; though certain aspects of social life, notably marriage, are described more fully. The basis of their political organization is said to be the village, *muyi*, under a headman, *mzee* (there is no 'tribal' chief), whose office is normally hereditary. There is a discussion of the lineage which, based on the *nasaba* or agnatic genealogy, is called *kabila*, a word that has to do duty also for 'tribe,' which is the normal meaning in Swahili. But though this double use of *kabila* has occasioned some confusion even among the Bajuni, the lineage seems no longer to have any real social function. The kinship system is dealt with all too summarily in four pages. The Bajuni are Shafi'ite Moslems; but any

local practices or deviations that may exist in their religion are not mentioned; and this part of the book, like the sections on political organization and kinship, needs further investigation.

The book is to be warmly welcomed, for it gives an excellent and well written account of an interesting people, and the material culture is adequately covered. We may hope that one day Professor Grottanelli will visit the Coast again and put us further in his debt by amplifying his record of the social and religious life of the Bajuni.

G. W. B. HUNTINGFORD

**Egypt.** By G. L. Harris. New Haven, Conn. (Human Relations Area Files), 1957. Pp. 370. Price \$8

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This up-to-date account of the situation in Egypt is impartial and so faithful to facts that the student for whom it is designed may find it a little difficult to see the wood for the trees. It is, however, clear that Egypt is spending £50 millions a year on armaments and that the cost of living is going up. In spite of the abolition of the Waqfs and the confiscation of the large estates, the fellah is no better off. Over-population is the rising tide that engulfs all attempts to better his lot. President Nasser is, however, no Canute. He is showing undoubted courage in his planned warfare against the desert on the north-western edge of the Delta where a new 'Liberation Province' is being created from sand. 600,000 feddans are down for reclamation with another 600,000 to follow, if the experiment succeeds. On them he proposes to settle in the end 1,000,000 people. Before being found worthy to wear the 'serviceable uniforms' of his Land Army, the picked fellah and his wife have to undergo strict medical examination. Five acres and a house are allotted to each family and loans are available at very low interest rates. So a class of super-fellahin is being created at a cost which the taxpayers in the other 15 provinces of Egypt show signs of grudging. So much for the light; shadows are cast by the fact that the new fields will require a good many coats of Nile silt before they will bear cotton or cereal crops. Also, the success of the larger scheme is dependent on the raising of the Aswan High Dam and to the cost of this the foreigner has as yet shown no alacrity to contribute. To him, no doubt, whatever blame there is to be ascribed will be duly passed on.

The book fairly gives the vanished British credit for the irrigation works of the past. All that is left for them to console themselves with is that Egypt still listens to the B.B.C.

G. W. MURRAY

## EUROPE

**How the Soviet System Works.** By Raymond A. Bauer, Alex Inkeles and Clyde Kluckhohn. Cambridge, Mass. (Harvard U.P.), and O.U.P. (Cumberlege), 1956. Pp. 274. Price £1 18s.

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Layman and specialist alike will be indebted to the Russian Research Center of Harvard University for having provided the first reliable detailed information on a subject of the utmost significance: the attitude of the individual Soviet citizen towards the society in which he lives. Since direct first-hand study is unfortunately impossible, the initiators of the 'Harvard Project on the Soviet Social System' set out to do the next best thing: to collect and analyse the testimony provided by the large number of Russians who left their homeland voluntarily or involuntarily during or after the Second World War. More than 10,000 émigrés were subjected to systematic and thorough questioning in order to establish their characteristic 'patterns of response.' The investigators were careful to take into account possible sources of bias on the part of the respondents, who varied widely in age, nationality and social and educational background.

The report demonstrates clearly the gulf that separates the objectives and ideals of the régime, which seeks to eliminate all potential rival foci of allegiance and insists on complete outward conformity, from the aspirations of the hard-pressed individual, who clings to his personal and family relationships as the last precious refuge from the all-pervading 'politicization' of daily life.

Why then, one asks, are there not more signs of overt disloyalty to the régime? From the thorough examination of this

problem presented here, two factors stand out. First, the régime relies heavily upon the support of the upper strata of society, who in return enjoy considerable material privileges, and thus have a vested interest in the perpetuation of the system. Technicians and managers may wish for less political interference in their work, but do not question the basic premises of the Soviet social and political order. This conclusion will surprise those in the West who still cherish the hope of a 'managerial revolution' in the U.S.S.R.

Another major factor making for social stability is the effectiveness of official propaganda, together with the general appreciation of Soviet achievements in social welfare and industrial construction. Especially amongst the young there is a greater tendency to blame defects upon the present men in power than upon the system as such—implying that with the passage of time the régime can be expected steadily to consolidate its hold upon society.

These conclusions seem perhaps somewhat over-pessimistic in the light of the part recently played by young people and members of the élite in pressing for change elsewhere in the Communist world—particularly in Hungary and Poland. On the other hand, one major aim of the post-Stalin leadership seems to have been to recruit greater popular support by a show of mildness in certain fields—almost as if they had read this report, compiled in 1954. There could hardly be a better tribute to the authors' accuracy in revealing the hidden tensions within Soviet society than to have their views confirmed from this unexpected quarter.

J. L. H. KEEP



**Gogmagog: The Buried Gods.** By T. C. Lethbridge. London (Routledge & Kegan Paul), 1957. Pp. xiv, 181. Price £1 1s.

## III

Mr. Lethbridge is in his element when tracking down unlikely items of folklore and survivals of primitive tradition; his chatty and discursive narrative is readable and persuasive. Whether it is also scholarly, I am not qualified to judge within the realms of folklore; certainly in Chapter I, Lethbridge blithely sweeps aside his historical sources when it suits him.

Apart from Gervase's account of the eleventh-century myth, the earliest actual mention of the figure is in two seventeenth-century writers, Hall and Layer, the latter of whom states that the giant's figure was 'within the said trench' (Wandlebury Camp) and was cut by the students of Cambridge. These definite statements are brushed aside by Lethbridge as they do not suit his theory. Now it may be surmised with Marples (*White Horses*, p. 205), that the ascription to students is due to a misunderstanding of Hall's Latin; but the placing of the figure within the camp is not found in Hall and is, therefore, an important original contribution, and one moreover which agrees with the eleventh-century legend. Layer also says that the figure is 'of late discontinued'; but it was still visible to Cole, c. 1724 (seemingly in the camp), though soon subsequently obliterated (and its traditions with it) by the construction of Lord Godolphin's house and gardens. These are facts: the vague local traditions which Lethbridge adduces to outweigh them, and to place the figure elsewhere, seem very slender by comparison.

But of course the crux of the matter is the excavation. Lethbridge claims to have found the figure, indeed more than the figure. The hillside is turning into a picture gallery, and the air photographs have been widely published. About this one must be frank.

Lethbridge has some hard things to say about 'archaeologists.' But many British archaeologists have long experience of excavation in chalk, and have developed techniques a good deal more subtle than those employed here. An excavation of this character and potential importance demands indeed the most sensitive techniques, and the most scrupulous presentation of the evidence. Of these there is no sign. Lethbridge has tackled his site with his usual slap-happy enthusiasm, and has taken no precautions, even of an elementary kind, to convince us even when he became aware of criticism. Where are the plans of areas carefully stripped? Where are the detailed photographs supporting his assertions on the nature of the fillings encountered?

The whole technique is founded on wishful thinking and upon a rejection of expert advice to the effect that these soft patches in the chalk are of glacial origin. Cut off the improbable-looking heads and one is left with a disjected system of frost pipes. The probe is

not a good instrument to determine accurately the edges of disturbances in the chalk. The only honest way to test this hill figure is to strip a large area and examine the disturbances in their setting. This has not been done. We are confronted today with a hill figure cut in the modern turf; only in some places does this digging go down into the chalk below, and where this happens it was clear to me (as to a C.B.A. committee which included Dr. Ian Cornwall) that the chalk cut into was of natural origin.

Furthermore, we have a basic contradiction between a figure outlined by turf-cutting and one with features moulded plastically in the solid chalk when, according to Lethbridge, these features (the breasts) were themselves concealed beneath the same filling as the rest. It is no secret that this filling has been examined by Professors Grimes and Piggott and Dr. Cornwall and considered by them to be due to weathering probably in Glacial times. There must remain more than an element of doubt whether these hill figures are earlier than the mid twentieth century; and once we doubt the hill figure, much of this book loses its point.

S. S. FRERE

### Volkskundliche Bibliographie für die Jahre 1937 und 1938.

Edited by Robert Wildhaber. Veröffentl. des Inst. für deutsche Volkskunde, Vol. VIII. Berlin (Akademie Verlag), 1957. Pp. xxxii, 543. Price DM. 38

## II2

Carelessly written paper slips, preserved throughout the war by the late John Meier, form the basis of this volume. The well-known Swiss editor mentions that some 90 entries from Nazi journals and newspapers have been excluded since they are of no interest for research purposes. He further expresses his gratitude for the correction and amplification of the entries by the members of the German Folklore Institute. Unfortunately their efforts did not cover the British section. Misprints such as 'Sasel' instead of 'Saxl' (Nos. 693 and 3833 and author index) are regrettable. Worse is the word 'presumably' in entry No. 1625; this should be absent from any bibliographer's vocabulary, especially in a case like this where one postcard would have cleared up the whole matter.

As the volume does not contain a list of contributors we do not know who is responsible for the sad state of British entries. Their total number amounts to some 40, where ten times the number would have been nearer the mark. Among the books omitted are, e.g., those by R. R. Marett, M. M. Banks's *Scottish Calendar Customs* and the various publications on English costumes. Whereas relevant papers from *Antiquity* and the *Warburg Journal* are listed, the equally important articles in *MAN* and *Folk-Lore* are missing. We can only hope that other countries fared better.

ELLEN ETTLINGER

### OCEANIA

**Söhne des tödenden Vaters: Dämonen- und Kopfjägergeschichten aus Neuguinea.** Recorded by H. Nevermann. Eisenach and Kassel (Röth), 1957. Pp. 225 (2), map. Price DM. 12.32

## III3

During an expedition for the Berlin Museum für Völkerkunde in 1933 and 1934, Professor Nevermann lived among the headhunters of southern Dutch New Guinea. Their main characteristics—excessive emotion and cruelty in warfare and ancient rites and their peace-time honesty, generosity and desire to learn and to travel—are briefly described. The old men are responsible for keeping alive the ancestral myths. Each family group traces its origin back to certain primæval demons, which are credited with the gradual creation of the world as it is today. During their ritual feasts the creation myths are performed in a dramatic and exact manner, which is believed to restore the spent life force. The Marind-anim, the largest tribe, are very attached to their special demons and their stories are powerful magic texts rather than mere folktales. Some demons are depicted in human shape; others as animals or trees. Though food taboos play an important part in the initiation rites, only among the neighbouring Jenan can be found traces of totemism.

After having gained the confidence of the Marind-anim and been adopted by the Kanum-irebe, Professor Nevermann was informed

about their traditional lore. The concise, explanatory paragraphs at the end of each story and the copious, more detailed notes printed at the end of the book, like the vividness of the eyewitness accounts, should appeal to a wide circle of readers.

ELLEN ETTLINGER

**Easter Island.** By Alfred Métraux, translated from the French by Michael Bullock. London (Deutsch), 1957. Pp. 249, 18 plates, 6 text figs. Price £1 1s.

## II4

*Easter Island* is a translation, and an excellent one, of the author's *L'Île de Pâques*, the 1951 edition of which was reviewed in *MAN* (1952, 135). Except in two respects, to be mentioned, the text is substantially unaltered, but the plates and figures are partly different and are an improvement over those of the French edition. The comments of the previous review, which were wholly favourable, apply equally to this translation.

Dr. Métraux has considerably expanded Chapter XII, 'The Mystery of the Tablets'; and Chapter XIV, 'The Origins of Easter Island Civilization,' replaces the few pages of 'Conclusion' in the French original. In Chapter XII the significance of the 'script' is discussed at greater length and the theories of Hévesy, Heine-Geldern, Imbelloni and others are examined. It seems now, as it



seemed in 1952, that Dr. Métraux's view that the tablets were no more than sacred mnemonics accords with the available evidence, but as he says a final conclusion must await the full publication of the important work being done by Dr. Barthel in Germany and by the Russians Kudriatsev, Oldorogge, Butinov and Knorozov. The caption of fig. 4, reproduced from *Anthropos*, is misleading; it is not made clear, as it is in *Anthropos*, that the columns of symbols are from the Indus Valley script, Easter Island and an ancient Chinese script respectively.

The last chapter consists partly of a criticism of the theories of Heyerdahl as they refer to the origins of the Easter Islanders, with side glances at those of Balfour and others. Heyerdahl's views are shown to be based on a selective use of evidence. Anyone doubting this should compare the use made by Heyerdahl of the chiefly genealogies (see *American Indians in the Pacific*, pp. 207f.) with Métraux's section on the same subject (*Ethnology of Easter Island*, B. P. Bishop Museum Bulletin 160, pp. 88-94 and Table 2). Dr. Métraux proceeds to show, by analysis of their food plants, domestic animals, ritual platforms, stone figures, etc., that the Easter Islanders are more likely to have come from the Marquesas than from Mangareva.

The appearance of this book in English is very welcome, and the extra material adds appreciably to its value.

B. A. L. CRANSTONE

**The Moa-Hunter Period of Maori Culture.** By Roger Duff. *Canterbury Mus. Bull. No. 1. 2nd edn. Wellington (Govt. Printer), 1956. Pp. xx, 400, 40 plates, 60 figs., 2 endpaper maps. Price £2 15s.*

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This important archaeological work was first printed in 1950 (see reviews by B. A. L. Cranstone, *MAN*, 1951, 196, and J. M. McEwen, *J. Polynes. Soc.*, Vol. LIX, 1950, pp. 85f.). The second edition remains substantially the same, but with important new material (though a reduction in type size has reduced the total number of pages, and pagination is altered throughout).

The book's basic thesis is supported by archaeological discoveries made since 1950. This thesis is now restated by Dr. Duff as follows (p. x): '... that the first settlers of New Zealand were Polynesians who found a numerous remnant of the unique Pleistocene bird fauna still flourishing in the South Island. The extermination of moa, swan, eagle and others required a long passage of time which was marked by the accumulation of cultural remains of distinctive and uniform types left by the Moa-hunters, particularly along the eastern seaboard of the South Island. The closest affiliations of this material culture beyond New Zealand are with Eastern Polynesia, particularly its marginal islands, while the Society Islands are implied as the cultural Hawaiki.'

The new parts comprise an extension of Chapter III, 'Narrative of Excavations' (p. 32), which includes a description of further Wairau excavations (p. 64), important new discoveries (p. 73), evidence on date of human occupation (p. 79), and the range of human association with the moa *Megalapteryx* (p. 80); another part entitled 'Man's role in the extinction of the moas' (p. 280) is added to Chapter VIII.

Especially interesting is evidence suggesting the recent survival of a small forest-dwelling moa in Takahe Valley west of Lake Te Anau. In 1949 Mr. Ken Miers, a wild-life officer of the Internal Affairs Department, discovered a cliff shelter where fossicking by Miers and a party led by Dr. R. A. Falla revealed bird remains, including those of *Megalapteryx*, in association with fowling's equipment comprising flax nooses, lures, etc. A report by Dr. Falla encouraged Dr. Duff and Dr. H. D. Skinner to visit the shelter in the hope of establishing stratification. Other important discoveries include the ploughing-up of a wealth of stone artifacts of characteristic Moa-Hunter types at Waitaki, and the discovery of an inland Moa-Hunter site on the northern fringe of Old Man Range, Central Otago, where there is evidence that moas were slaughtered by man in great numbers.

The results of radio-carbon analyses for the Wairau Bar and the Takahe Valley have generally supported chronological assumptions made earlier on cultural grounds, viz. that the Moa-Hunter phase of Maori culture (ideally represented at the Wairau Bar) belongs to

the pre-Fleet era, and that the Takahe Valley finds are of immediately pre-European date. The limitations of this dating technique for the recent past are recognized, but it is interesting to note that radio-carbon dates<sup>1</sup> provided from duplicate Wairau Bar samples independently submitted almost overlap (A.D. 905 to 1125/A.D. 1175 to 1275), thus agreeing with a general chronology provided by the evidence of tradition and by inference from cultural material. Dr. Duff says (p. xii): 'If we take the twelfth century as the overlap of the two determinations, and A.D. 1150 as the mean point, we may regard the Wairau occupation as contemporary with the arrival of Toi-kai-rakau, the oldest New Zealand ancestor whom many lines recognize and who is dated, by averaging family trees, to A.D. 1150.'

The influence of Dr. Duff's work on archaeology in New Zealand has been far-reaching. It has not only inspired enthusiastic interest in serious archaeology, but discouraged indiscriminate fossicking and 'curio-hunting.' The book has become an acknowledged classic within a few years of publication, and has the added distinction of wide popularity, for it has found a place on the bookshelves of farmers, schoolboys, collectors and teachers, as well as in the museum library. We may agree with Dr. H. D. Skinner's foreword that this, New Zealand's first archaeological book, 'opens a new horizon in Polynesian culture history.'

T. T. BARROW

Note

<sup>1</sup> Determinations made in 1955 respectively by Dr. E. S. Deevey at Yale and by Dr. T. A. Rafter at Wellington.

**Bibliography of Oceanic Linguistics.** Compiled by H. R. Klieber. London (O.U.P.), 1957. Pp. xiii, 143. Price £2 5s.

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Professional linguists, students and research workers will welcome this remarkable bibliography. It contains 2,100 references and covers the whole of Oceania—Polynesia, Melanesia, and Papuasia, but not Australia and the Philippines. It takes interest in linguistic works as such: dictionaries, grammars, individual or comparative studies, while leaving aside the extensive literature, chiefly religious, published by various Christian Missions in many South Sea languages. The work proves a serious one, following the best bibliographical methods and literally exhaustive. The author has himself run through the great majority of the works he quotes. He mentions where the rarer books are to be found. One might regret that M. Klieber has failed to add to each reference a rapid summary of its contents and an objective appreciation of its scientific value. Yet such regret is all in favour of the author who has enriched works of reference concerning Oceania with a book which will stand out and which with its solid and brilliant qualities opens the series of the 'London Oriental Bibliographies.'

PATRICK O'REILLY

**Unter roten Hibiskus-Blüten.** By L. Kohl-Larsen. 2nd edn. Kassel (Röth), 1957. Pp. 144.

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The first edition of this book (this one has been revised) was written soon after the First World War by the German Deputy-Governor of Yap, Western Carolines, who lived as a doctor on this South Sea Island before the war broke out. Most of the information given by the author no longer applies to the present situation, utterly changed as it has been by the events of two world wars and 25 years of Japanese occupation. The book, which is written in a popular style, is likely to fall between two stools. The general reader may be tempted to abandon it before his appetite for the exotic is satisfied since public taste has become somewhat tired of nineteenth-century 'South Sea Island romanticism.' The anthropologist, on the other hand, may regret the tendency to over-generalize. The data, it would seem, are there, but it needs a patient reader to extract the real meat.

Nevertheless the book, which is essentially descriptive rather than analytical, is informative, especially as far as the medical and religious practices of the Yap Islanders are concerned. These have today to a great extent vanished and the descriptions are therefore invaluable to students interested in the historical background of acculturated societies and comparative studies.

B. R. STILLFRIED









(a) Qalanders on the move with their tents at Khalghat, Central India



(b) Qalanders at Indore



(c) A Surrey Gypsy camp



(d) A New Forest Gypsy camp



(e) A Skye tinker's tent

# THE GYPSY BENDER TENT AND ITS DERIVATIVES

Photographs: James Walton (a, b, e), Sir Arthur Clay (c) and Harold Bastin (d)



# THE GYPSY BENDER TENT AND ITS DERIVATIVES\*

by

JAMES WALTON, F.S.A.

Maseru, Basutoland

**II8** In October, 1944, near Khalghat, in the Narbada Valley, Central India, I came across a party of Qalanders, nomadic Muslim showmen from Gwalior. Some were already seated astride their bullocks, which were piled high with tent frameworks, tent cloths, cages of fighting quail and partridge, babies in rope cradles and a miscellaneous assortment of cooking utensils and family belongings (Plate Ha). Some had still made no attempt to dismantle their tents, and the women, with their dark flowing hair and Gypsy features, squatted on the ground and smoked a hookah.

On my approach the menfolk began their show by taking two fighting quail from their home-made cages. These quail, and the partridge, were obtained from the Pardhis, a wandering caste of hunters and fowlers who practise a low form of Hinduism bordering on animism. Another older member of the party tied his long hair to a heavy stone which he proceeded to lift, whilst a sturdy youth knelt on the ground and received the full force of a charge at speed from his trained ram. A year earlier at Indore I had seen a similar troupe of Qalanders, whose entertainment included performing bears, fighting monkeys and a snake-killing mongoose, in addition to the quail and partridge. The women in both cases busied themselves in making simple toys from reeds and pith.

Whilst the Qalanders travel from village to village and are fairly frequently encountered, the Pardhis spend much of their time hunting and trapping in the forests. Near Asapura, in the Vindhya Mountains, I saw a Pardhi camp. The hunters had just arrived on their laden bullocks at a clearing in the forest and were busy erecting their tents, *tambu* or *raoti*. The Pardhi *tambu* is identical with that of the Qalanders (Plate Hb). Six hoops of twisted withies are set in the ground about one foot apart with a low mud wall, six inches high, along each side. At one end an extra curved member is tied in place and the framework is covered with a tent cloth, to provide a shelter about four feet high in the centre. The baby is suspended in a hammock cradle from the members of the framework. During travelling, the tent cloth is rolled up and the ends of the hoops are pulled together with cords and tied to the back of a bullock.

The simple *tambu* of the Gypsies of Central India is a fundamental shelter framework which may easily have originated independently in a number of widely separated areas but it does seem to be peculiar to peoples of Gypsy type, and in Central India I did not find it in use among other nomadic tribes. The bender tent is familiar in Britain as the typical shelter of the Gypsies, particularly in the New Forest and in Surrey. A Gypsy tent in Surrey, photographed by Sir Arthur Clay between 1872 and 1890, had a framework of five main hoops which was covered with a tent cloth and had an entrance opening in the middle of one

side.<sup>1</sup> The hearth was placed in the centre of the floor and the big iron cooking pot was suspended from a swan's neck hook driven into the ground. In fine weather the tent cloth over the hearth was rolled back and the hoops released, but a portion of the tent cloth was retained at one side to serve as a screen for the fire (Plate Hc). An identical construction and arrangement is shown in Mr. Harold Bastin's photograph of a New Forest Gypsy tent (Plate Hd). In this tent the fire was built on a raised iron grate but the same device was employed for hanging the cooking pot.

The bender tent is the home, too, of the tinkers and basket-makers in the highlands and islands of western Scotland, particularly on Skye and the adjoining mainland. The Scottish tinkers are strongly Gypsy in appearance and character and their tent is very similar to that of the Gypsies in southern England. The central hearth is today frequently replaced by a closed iron stove with a chimney leading out through the top of the tent (Plate He).<sup>2</sup>

In a more developed and permanent form the bender tent has about as sporadic a distribution as the barrel-vault hut. It is the dwelling of the Bateke in the Congo, where the huts are built in rows with their backs to the river and the fronts, with their tiny rectangular doorways, facing onto the street. The Basonge also have huts of this type<sup>3</sup> and the Masai *tembe*, on the opposite side of the continent, belongs to the same group. The latter 'can best be compared with a round-topped trunk. Though the Masai usually stand well over six feet, their huts, which (quite conformably with the owners' mode of life as cattle-breeders *par excellence*) are neatly and fragrantly plastered with cowdung, are so low that even a person of normal stature cannot stand upright in them.'<sup>4</sup> 'The women do the work of building. They procure poles, and put one end in holes, which they dig in the ground; they then bend the poles together with cord made from trees; after which they cover the frame with long grass. When they have finished this, they plaster the whole of the outside with cowdung and mud.'<sup>5</sup> Frobenius has illustrated a large and beautifully decorated Barotse chief's hut from Sealni, Northern Rhodesia, which also has a framework of semicircular hoops.<sup>6</sup>

The framework of the reed huts, *sarifa*, of the marsh areas of Southern Iraq consists of five or more bundles of reeds bent into semicircles and held together by eight other bundles of reeds crossing the hoops at right angles at equal intervals. The framework is then covered with reed mats.<sup>7</sup> Frobenius illustrates a similar hut used by fishermen on the Niger in the Bamako district. This has six hoops held together by eight horizontal poles and covered with woven grass mats. The doorway opening is in the centre of one side. The example figured has a length of 4.20 metres, a width of 2.05 metres and a height of 2.20 metres.<sup>8</sup> In India the Toda of the Nilgiri Hills live in 'barrel-vault' huts which are very similar indeed to those of the Congo

\* With Plate H



Bateke, not only in fundamental construction but also in having a recessed end wall providing a sheltered entrance and in having a small rectangular entrance set asymmetrically in the recessed wall.

From such records as I have been able to collect,<sup>9</sup> the 'barrel-vault' type of construction, whether as a tent or as a more permanent dwelling, appears to have originated in a number of widely separated areas. This is to be expected with such a simple dwelling type. What is more remarkable is that, although it is a very convenient shelter for nomadic peoples and affords far more space than a circular hut as a permanent dwelling, it has rarely spread to neighbouring tribes from those who first invented it. Edlin has suggested that the bender tent 'may have inspired the barrel vaults in Norman churches,'<sup>10</sup> but it is only during the present century that this type of construction has really taken its place in the architectural world to any extent.

In conclusion I would like to thank Rachel, Lady Clay,

and Mr. Harold Bastin for allowing me to reproduce their photographs of Gypsy encampments which constitute a valuable ethnological record.

#### Notes

<sup>1</sup> Rachel Clay, 'Gypsies of the Surrey Hills,' *The Countryman*, 1952, p. 295.

<sup>2</sup> See also Dorothy Hartley, *Made in England*, 3rd edn., 1951, pp. 160-2 and Plates XXXIX, XL, for a description of a Skye tinker's camp.

<sup>3</sup> James Walton, 'South African Peasant Architecture,' *African Studies*, Vol. VIII (1949), pp. 76f. and Plate IV.

<sup>4</sup> Karl Weule, *Native Life in East Africa*, 1909, p. 86.

<sup>5</sup> A. C. Hollis, *The Masai*, 1905.

<sup>6</sup> Leo Frobenius, *Kulturgeschichte Afrikas*, 1933, Plate CLI.

<sup>7</sup> Wilfred Thesiger, 'The Marshmen of Southern Iraq,' *Geog. J.*, Vol. CXX, Part 3 (1954), p. 276.

<sup>8</sup> *Ibid.*, fig. 173.

<sup>9</sup> I should be very grateful to hear from readers of any other examples of this type of construction.

<sup>10</sup> H. L. Edlin, *Woodland Crafts in Britain*, 1949, p. 137.

## A NOTE ON CENTRAL AFRICAN DREAM CONCEPTS\*

by

J. H. CHAPLIN

Livingstone

**119** Some time ago, Hodgson, writing in *MAN*,<sup>1</sup> contributed notes on dreams and their interpretation by headmen of tribes in the Dowa District of Nyasaland. The following information refers to a related tribe in Northern Rhodesia, the Nsenga of Petauke district.<sup>2</sup> The headman of a village, who was well known for his wisdom in understanding dreams, dictated to his grandson his method of interpretation, and I have had access to the collection of notes. His method included an appreciation of the shapes of some dreams as well as their content. Fig. 1 shows the main patterns which he recognized. It will be seen that in some instances the pattern is representative of movement in space, in others of the passing of time. I have marked parts of the designs in small letters to simplify the reference to points.

My informant, the grandson, looked to guidance from dreams in all his everyday affairs; usually it was the spirit of his grandfather (less frequently his grandmother) who spoke to him. This he considered to be due to the power of their spirits; people with departed relatives of less wisdom received their guidance at second-hand in dreams of the type that will be described. A typical 'direct-guidance' dream was this: 'A high shrill voice was heard and the dreamer told to stand up with his hands held palms upwards at face level, he was then directed to count slowly. At "four" the figure of his absent wife appeared standing in miniature on his hands, she stayed briefly then faded and the dream ended.' This was taken to indicate that she would arrive in four days' time; it should be recorded that she made a previously unexpected return at that time.

\* With a text figure

In the following account, Roman numerals refer to the patterns in Fig. 1, small letters to points on those patterns.

**i Dream:** There is no change throughout the dream (a-b), either dancing or singing or something else pleasant all the time; but on waking you can remember little more than a general feeling of pleasure.

**Interpretation:** This is a bad dream: for a man because his yet unborn child is likely to be stillborn or deformed, for a woman that a child may die.

**ii Dream:** A pleasant dream to begin with, often with music, but there is a change (b) and a friend will interrupt, but he goes away (c) and the dream goes on as before.

**Interpretation:** The man who comes into the dream may appear to be a friend, but really means to cheat you in some way.

**iii Dream:** You are travelling upwards towards Heaven (a-b) but at the top you see something unpleasant and drop down (b-c). You fail to reach the bottom (c-d) and dream of something different.

**Interpretation:** You are likely to have a very serious sickness, but will eventually recover.

**iv Dream:** You dream that you are very sick, or dying (a-b) but there is a change (b-c) and pleasant things happen. You suddenly meet a woman (c) and she agrees to your advances (c-d), and afterwards you dream of killing somebody.

**Interpretation:** You are being warned that people are jealous of you (a-b); the change (b-c) indicates the coming of a gift. An emission at (d), or a sensation of it, indicates virility, while the closing scenes of death (d-e) emphasize the warning of jealousy.

**v Dream:** You dream of going up into Heaven (a-b) because of bad things in your life on Earth. At the top (b) you meet a man who tells you to go down again and bring him a certain thing. You descend (b-c) and at the bottom see what you have to collect and take it up to him (c-d).

**Interpretation:** This is a very dangerous dream. When you come down you must try to wake yourself up and finish the dream by stroking the back of your head. If you do not do this, what you are really doing is to take your life and your friends will come





FIG. 1. SOME NSENGA DREAMS

in the morning and find you dead. (The interpretation of this dream comes through the use of a *chitumwa* or through the direct information of the ancestral spirit in a dream.)

**vi Dream:** As in v things are bad so you go up into Heaven (a-b); up there you either see good things or a dead friend tells you of something, perhaps a tree. You descend (b-c) with the good things and at once waken.

**Interpretation:** A good dream; on waking you should seek whatever it was you seemed to bring down, or the particular tree. Then put the object or piece of root under your pillow and sleep again, and the same man will come and give you valuable knowledge of medicines. These will stay clear in your mind until waking.

**vii Dream:** In your dream you fall asleep and dream and can see yourself asleep; a friend comes and wakes you, disturbing the 'inner dream' and you crossly tell him you have forgotten everything in it.

**Interpretation:** A dream that few people have, it is a teaching dream and means that in the future you will be told useful things, but must keep them secret.

**viii Dream:** You are going on a journey (a-b) and meet a friend and turn off the road to visit his house (b-c); after talking you return (c-b) and continue (b-d) and meet another friend later and visit him also (d-e). Alternatively, a dream suddenly changes, after a while reverts to the first theme, changes again, and yet again reverts.

**Interpretation:** A good dream for young married people. If you visit a male friend (or the interruption is a male thing, such as hunting), your next child will be a boy; if a female friend (or female things—cooking or pounding meal), the child will be a girl.

**ix Dream:** You are splashing about and swimming in water (a-b), then you come out and fly away like a bird (b-c).

**Interpretation:** You will have a long and healthy life.

**x Dream:** As in ix, you are swimming in water, but go underneath the water and walk about and live quite happily there.

**Interpretation:** A bad dream, as it means that you will soon die.

**xi Dream:** You are bathing at one end of a large pool (a) at the other end of which is a path (x-y) leading to your house. You see a group of naked men or boys coming towards you, so swim away and jump out onto the bank (b); there is another group there, so you jump back; this happens again (c, d) until you eventually reach the far end (x) and run to your house.

**Interpretation:** Your house is being attacked by witchcraft as you sleep, but the man responsible cannot get in because of your counter-magic, probably a *chitumwa*. You should wake up and go outside, suitably protected; he will be unable to move and you should beat him, but not so hard as to cause blood to flow, otherwise you will have a wound in the same place as your victim.

**xii Dream:** You (or your hut) are surrounded by fire.

**Interpretation:** As in xi, witchcraft is around; if you have a *chitumwa* use it, if not light a candle and let it burn all night and this will keep evil effects away.

Some other dreams for which no patterns were suggested may be given here.

(1) Catching white fish: on waking look carefully for a river, or if it is too far, look on the path to it and you may find something good, otherwise it is a dream of long life.

(2) Catching black fish: a member of your family has died, or you will die within a year.

(3) Walking shirtless with a friend: a fight will take place between you and your companion.

(4) Eating: this is bad as it generally means that there is, or will be, an attempt to poison you; you must eat the roots of the trees or plants to counteract this.

(5) Walking naked: an attempt will be made to bewitch you.

(6) Erotic dreams: a solely erotic dream whether with an emission or not is of little importance, the body is 'just practising' (but see iv above). It seems that the identity or even the sex of the person contacted has little or no effect on this decision. Bathing in a group of naked people is also without significance (though mixed groups are not sanctioned in village life), but to be the only man bathing in a group of women shows that you are likely to be sterile.

(7) Leaves: these indicate that the dreamer will have some letters.

(8) Snakes in trees: on waking look for the same kind of tree as that in the dream; the charred and pounded roots inserted into cuts on the wrist will protect against snakebite.

(9) The sun: to dream of the sun is meaningless as it is too bright to indicate anything.

(10) The moon: a dream in which the moon figures will always be true, and often it has to do with money.

(11) Water: to dream of dropping something into a river or pond indicates that you will soon suffer loss.

(12) Maize: however many maize cobs you see on a single plant is the number of years you will keep your job.

(13) Red water: this indicates that the dreamer's wife has conceived and her monthly periods will cease; conversely, to dream of a woman stopping her periods means that she has not conceived.

(14) Colours: a predominantly red colour in dreams means death; white is a lucky colour.

(15) Conversations: if you are asked questions in a dream, especially if the questioner is not a very reliable friend or close relative, always answer 'No' or make foolish answers, to avoid trouble.

In comparing these interpretations with those of Hodgson mentioned above, we can see one or two coincidences. The dream of falling through space (iii) has an unpleasant interpretation in his No. 15 as well. The dreams of fish (my 1 and 2) are similar to his No. 18, and the dream of flying (ix) is connected with good fortune as is his No. 1.

It will be noticed that some dreams indicate their opposites. For example the even pleasantness of 1 foretells misfortune; the friend in ii is a deceiver; ability to live under water in x indicates death while in 13 the red water represents the menses which have ceased. That interpre-



tations are not all related to pre-European events is shown in the connexion of the moon with money, and maize with jobs.

That there was any difference in interpretation according to the sex of the dreamer was not mentioned. It seems that the erotic dream of a man can do no harm, but it should be recorded that for a man to attempt intercourse with a woman while she is asleep is very dangerous, because she will dream that such an action is taking place. If the man she dreams of is living he can add your sexual powers to his own, leaving you impotent; if she dreams of a man who has died, then his spirit can take your life away and you will be dead by morning. Amongst the Malozi and Batonga, tribes of the south and west of the country,<sup>4</sup> this is a punishable offence by tribal law, and if the woman reports it to her brothers, the husband can be fined in the tribal court. I have, at present, only a little information concerning the frequency of erotic dreams or their contents, and also no evidence as to whether women have fewer such dreams than men, as is the case in more advanced societies.<sup>5</sup>

Perhaps the most interesting dreams in this brief collection are v and vii. The latter suggests that it is possible for the dreamer to stand aside and watch his own dream, while the former indicates that, though sleeping, a man can to a certain extent control his dream, or at least can bring about his waking. Incidentally the informant was convinced that the easiest way to efface nightmares from the memory on waking was to stroke the back of the head, the action to be used in dream v to waken the dreamer.

In conclusion, it is perhaps worth recording a few notes on Batonga dreams.

- (1) Snakes: an ancestral spirit.
- (2) A big river: an ancestral spirit.
- (3) Pots: black = rain; white = good luck; red = fighting.
- (4) Encircling fire: witchcraft, but if you jump out of the fire you are safe.
- (5) A hare: witchcraft.
- (6) Drinking beer: witchcraft.
- (7) An empty hole: a grave which either you or a close relative will soon occupy.
- (8) A hole full of water: jump in; if you emerge quickly you will have good luck, if slowly, you will have misfortunes.
- (9) Flying: good news will soon come, but dropping quickly in flight is bad.
- (10) Eating: to eat bananas is good, to eat other foods is bad, especially meat, which may be the human flesh given by witchcraft practitioners. To defecate in a dream just after eating is a good sign as it means you have overcome the badness.

#### Notes

<sup>1</sup> A. G. O. Hodgson, 'Dreams in Central Africa,' *MAN*, 1926, 39.

<sup>2</sup> Living about latitude 14° South, longitude 31° East.

<sup>3</sup> J. H. Chaplin, 'On the Making of a Chitumwa, a Northern Rhodesian Protective Amulet,' *MAN*, 1957, 184.

<sup>4</sup> The Batonga live about 17° South, 27° East, while the loosely termed Malozi tribes are centred about 15° South, 23° East.

<sup>5</sup> A. C. Kinsey and others, *Sexual Behaviour in the Human Female*, Philadelphia, 1953, p. 215. The table shows that there is a smaller incidence of such dreams for females than for males, but it is likely that the quite different approach to sexual relations of the rural African woman may alter this ratio.

## SHORTER NOTES

### A Symposium on Human Biology. A Report by Dr. G. Ainsworth Harrison

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Although physical anthropology has been recognized as a separate subject in the academic activities of this country for a long time, it has been so transformed in aim and method in the last few years that a redefinition of its scope has become necessary. Only when those in allied subjects are familiar with its purpose and achievements can its rightful position be gauged. It was with these objects in view that a symposium was held on 6 November, 1957, at the Ciba Foundation, London, with the title 'The Scope of Physical Anthropology and Human Population Biology and their Place in Academic Studies.' The initiative for arranging the meeting was taken by the Council of the Royal Anthropological Institute and Dr. J. S. Weiner organized the programme in collaboration with the Ciba Foundation. It was attended by nine human anatomists, six physical anthropologists, six human geneticists, three social anthropologists, two workers in social medicine, representatives from the Medical Research Council and the Director and Assistant Director of the Ciba Foundation. Dr. J. A. Fraser Roberts was the chairman for the morning session and Dr. K. P. Oakley, F.B.A., for the afternoon one.

The first paper, 'Reorientations in Physical Anthropology' was fittingly given by Professor Sir Wilfrid Le Gros Clark, F.R.S., since he has been largely responsible for nurturing the modern development of the subject. After describing the various phases through which physical anthropology has passed, he pointed out that, although it remained in part a historical subject, future

'research will be mainly directed to the study of living populations, to problems of human genetics, of the relation of physical characters to the environment, the study of growth processes and the study of comparative racial physiology.' He therefore believed that 'the physical anthropologist, if he is to justify his subject as a separate scientific discipline, with its own special problems and its own technical methods, must be primarily a field worker.'

Advances in genetics in general and human genetics in particular have had the greatest single influence on the recent development of physical anthropology. Much of the research now being done in the subject concerns human population genetics and Professor L. S. Penrose, F.R.S., in his paper 'Human Variability and Adaptability' predicted that anthropology would become largely transformed into 'human race genetics.' He pointed out that one of the most important characteristics of a modern genetical analysis is 'the attempt to estimate the direction and speed of gene changes' and stated 'that surveys of genetical anomalies are required as well as surveys of so-called normal characters.' This point was driven home by Professor A. C. Stevenson in his paper 'Biological Studies of Small Communities' in which he demonstrated, by reference to his own authoritative study of deaf-mutism in Northern Ireland, the approach to and methods of a demographic survey.

The afternoon session was opened by Professor J. Z. Young, F.R.S., on 'Physical Anthropology as a Liberal and Scientific Discipline.' He put forward the plea that 'anthropology should not be parochial but of a much grander nature' and considered that it 'provides as firm a foundation as any other for a liberal education.'



It puts the various factors that control human behaviour into their right proportions.' He believed, in particular, that 'for introducing medical students to the complexities of life it is extremely valuable, because it helps them over the boundary between physical science and the cultural sciences without a serious break.' Professor Young also pointed out that too much emphasis must not be placed upon genetic research and that 'the study of learning powers and their distribution might be as important.'

Speaking on 'The Experimental Approach to Physical Anthropology' Dr. N. A. Barnicot said that, although experimentation had indeed been of value in tracing the most probable course of primate and human evolution, the method had the greatest relevance to the study of biological variation of living populations. He presented a large number of interesting examples, genetical, physiological, biochemical, ecological and behavioural, in which the experimental method either had been or could be profitably used.

Because the genetics of many characters of human blood have now been worked out and because there is ever increasing evidence that these characters are extremely important in adaptations to particular ecological conditions, Dr. A. E. Mourant concentrated mainly on the methods for collecting, transporting and testing human blood in his paper 'Organization for Field Research.' He noted that 'the tendency now is more and more for the specialists in the various fields to co-operate in the study of particular populations.'

In his paper, Dr. J. S. Weiner dealt with 'Courses and Training in Physical Anthropology and Human Biology.' He pointed out that there are only some ten centres where active research in physical anthropology is being undertaken and that 'the total qualified personnel associated with these centres probably does not exceed 25 in all.' He further noted that only 'five are in a position to make the kind of sustained teaching impact which is a basic requirement for future progress.' In view of the fact that there is not only a need for professional physical anthropologists (in both pure and applied research) but also for the subject to be taught at an elementary level to students of medicine, dentistry, social anthropology, archaeology, ethnography, human geography and colonial affairs and, certain facets of it, at an advanced level to human anatomists, zoologists and students of social medicine, Dr. Weiner felt that there were too few University posts for physical anthropologists. He concluded that since many human anatomists are doing research which could reasonably be called human biology 'the most useful single practical step (to help the subject) that could be taken in Great Britain would be the expansion of teaching of physical anthropology at both elementary and advanced levels in departments of anatomy.'

Finally, a short paper on 'Physical Anthropology in the British Museum (Natural History)' was given by Dr. K. P. Oakley, F.B.A. He showed that there was a general demand for a single centre to function as a repository for material of anthropological interest (skeletal material, X-ray photographs, data cards, etc.) and said that the Museum was prepared to accept this role. He also briefly reviewed the research activities of the Anthropological Section of the Museum.

Dr. J. M. Tanner was unfortunately unable to attend the Symposium, but has presented a paper for the published proceedings entitled 'The Place of Human Biology in Medical Education with Particular Reference to Studies of Human Growth and Constitution.' In this he states that medicine should be taught as 'applied human biology,' and offers a curriculum for a preclinical course, centred on three major subjects, 'Cellular Physiology,' 'Mammalian Organization' and the 'Organization of Man.' In this course, genetics, ethology, growth and general pathology are integrated with the more usual preclinical subjects. Dr. Tanner

also suggests that English and Mathematics be made compulsory subjects for medical students at the advanced G.E.C. level.

Each paper was followed by a general discussion. The day's proceedings were summed up by Professor Sir Solly Zuckerman, F.R.S., who, on behalf of all present, thanked Dr. J. S. Weiner for organizing the Symposium. While integrating many of the points made in the papers and discussions, he expressed concern that the Nuffield Blood Group Centre was in jeopardy and supported the suggestion that a group of professional human biologists should be established. (This is now in the process of being formed.) Finally, he said that 'physical anthropology must become professional; it must get rid of the slight tarnish of amateurism.' 'My own view is that we have simply got to face the fact that if the subject is going to live, it can only live today as part of the physical basis of demographic studies.'

Arrangements are being made to publish the papers presented at the Symposium.

### Stone Implements from the Rub'al Khali, Southern Arabia.

By Henry Field, M.A., D.Sc.Oxon., Research Fellow, Peabody Museum, Harvard. With two text figures

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Surface stone implements have been collected by the staff of the Arabian-American Oil Company (Aramco) within the triangle bounded by the Wadi Dawasir—Bahr as-Safi—Umm Gharib in the south-western Rub'al Khali.<sup>1</sup>

A new site (Station 13) located at Lat. 18° 17' 39" N. and Long. 47° 06' 58" E. was found on 9 November, 1956. This ancient camp stands on a broad gravel plain with remanée of an old lake (?) deposit surrounded by large moving sand dunes. On 13 January,

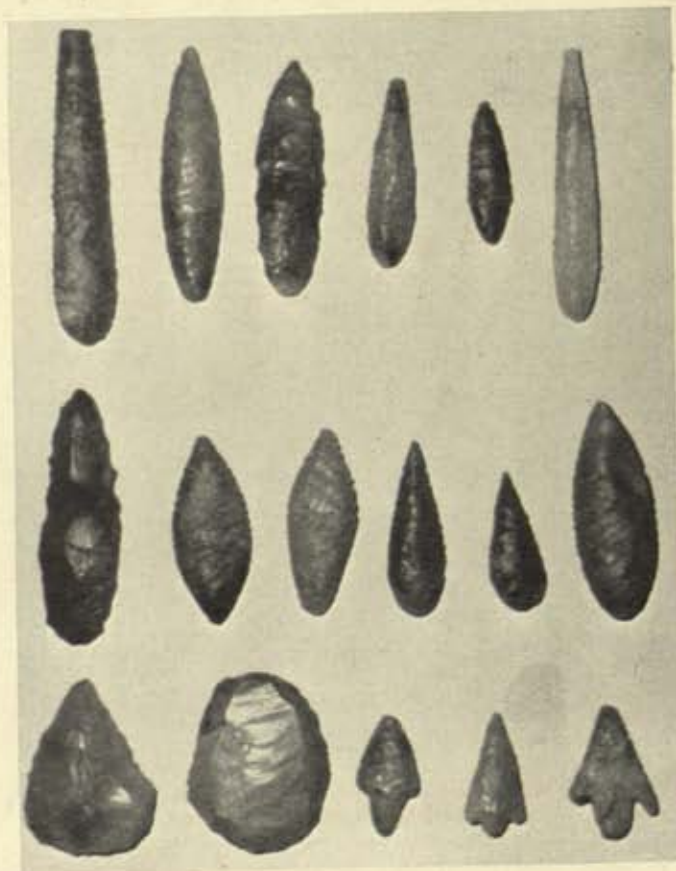


FIG. 1. FLINT IMPLEMENTS FROM STATION 13



1957, Mr. O. A. Seager made a collection of flint and quartzite tools and flakes (figs. 1 and 2), which he presented to the Peabody Museum, Harvard. This surface station lies equidistant from the rich sites at Jiladah ( $18^{\circ} 48' \text{ N.}$  and  $46^{\circ} 18' \text{ E.}$ ) and the Aramco camp ( $18^{\circ} \text{ N.}$  and  $48^{\circ} \text{ E.}$ ).

The implements include arrowheads, points, Solutrian-type *feuilles de laurier* and one discoidal scraper. The material is flint, chert and fine-grained quartzite. The surface which lay exposed was polished by the wind and sand; many specimens possess 'desert varnish.' The flint and chert implements range in colour from milky white, light yellow, grey, pink to dark brown. The quartzites tend to be dark reddish brown, but there are some pink and

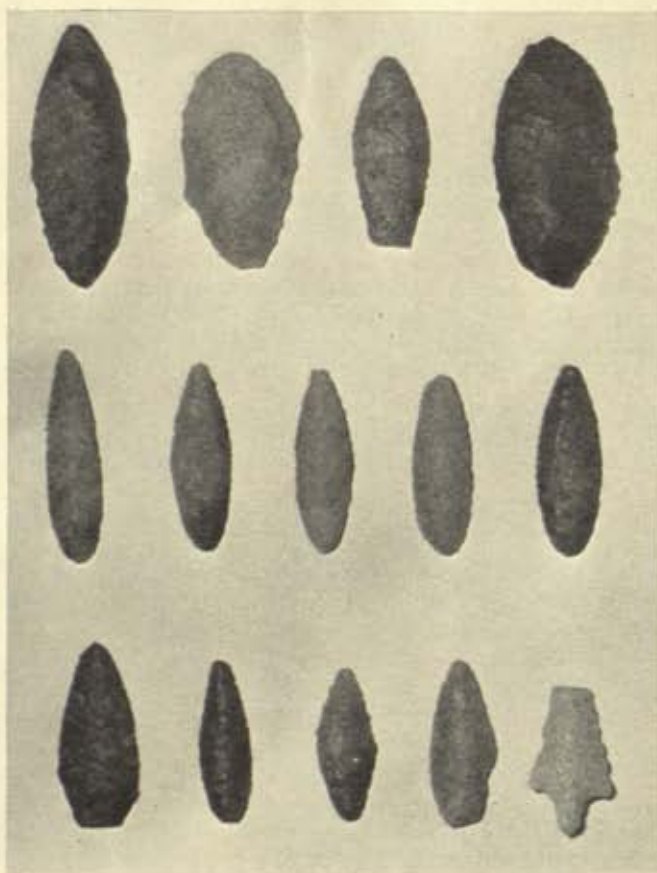


FIG. 2. QUARTZITE IMPLEMENTS FROM STATION 13

some yellowish pink points. The thinness of the blades combined with the delicate retouching indicates excellent pressure-flaking techniques, reminiscent of Ancient Egyptian, Western European and Danish craftsmen. Since no stratified deposit has yet been found in this area, no dating is possible. However, these implements may be assigned temporarily to a 'Neolithic' cultural phase.<sup>2</sup>

Since the completion of my map (No. 24) of the Archaeological Sites of Arabia,<sup>1</sup> two new surface stations have been found by Aramco geologists and surveyors. The following notes were supplied by Mr. O. A. Seager on 7 December, 1956:

1. Station 6 ( $18^{\circ} 45' \text{ N.}$  and  $46^{\circ} 10' \text{ E.}$ ) on 16 May, 1955. This locality, five miles east of Jiladah, lies amid isolated gravel patches with much petrified wood between large sand ridges. The country opens to gravel plains 20-30 kilometres to the west.

2. Station 7 ( $17^{\circ} 20' 30'' \text{ N.}$  and  $47^{\circ} 10' 30'' \text{ E.}$ ) on 17 May, 1955, at Shaurah, a large embayment on a sandstone outcrop amid gravel plains surrounded by dune sand opening to plateau country to the south.

It seems probable that this station and the others already found within the great Rub'al Khali served as camp sites for the ancient hunters, who sought the oryx, gazelle tahr, cheetah, wolf, hyena, jackal, fox and ostrich.

As each new site is added to the mosaic, the general picture becomes clearer. The greatest gap is from Aramco camp G-2554 ( $18^{\circ} 18' \text{ N.}$  and  $49^{\circ} 46' \text{ E.}$ ) in the Umm Gharib desert eastward to near Awai<sup>3</sup> in Wadi Amairi—a distance of 500 miles.

It is hoped that future discoveries in this area will be published so that valid deductions may be drawn.

#### Notes

<sup>1</sup> H. Field, *Ancient and Modern Man in Southwestern Asia*, 1956, University of Miami Press, Coral Gables, Florida, pp. 98f. and Appendix D, pp. 181-89, to accompany Map 24 (Archaeological Sites of Arabia).

<sup>2</sup> F. E. Zeuner, "'Neolithic' Sites from the Rub'al Khali, Southern Arabia," *MAN*, 1954, 209, with bibliography; H. Field, 'New Stone Age Sites in the Arabian Peninsula,' *MAN*, 1955, 145.

<sup>3</sup> See *MAN*, 1955, 145.

#### The Institute of Race Relations

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A new body entitled the Institute of Race Relations has been formed 'to promote, encourage and support the study and understanding of, and the exchange of information about, relations between different races and peoples and the circumstances and conditions in which they live and work.' The Chairman of the Council of the Institute is Sir Alexander Carr-Saunders, and Mr. Philip Mason has been appointed its Director.

The Institute takes over the work on race relations hitherto conducted in the Royal Institute of International Affairs (Chatham House), which began when Mr. Mason was appointed Director of Studies in Race Relations at Chatham House in October, 1952. It was then envisaged that a separate Institute would eventually be formed and the original intention has now been fulfilled. The Registered Office of the new Institute is at 6, Duke of York Street, London, S.W.1 (telephone: Trafalgar 4161).

#### Closing of the Department of Archaeology, Carnegie Institution of Washington. Communicated by Dr.

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H. E. D. Pollock, Director  
As indicated in recent annual reports, the Carnegie Institution of Washington is withdrawing from the field of Middle American archaeology. On 1 July, 1958, the offices of the Department of Archaeology, at 10 Frisbie Place, Cambridge, Massachusetts, will close, and the Department will cease to exist. After that date all correspondence concerned with the past activities of the Department should be addressed to the Carnegie Institution of Washington, 1530 P Street, N.W., Washington, 5, D.C.



## CORRESPONDENCE

The Seligman Mask and the R.A.I. Cf. MAN, 1957, 143

**124** SIR,—Many readers of MAN will have seen press reports of the sale by Mrs. Brenda Z. Seligman of her Benin ivory mask. She is setting aside the proceeds, some £20,000, as a memorial fund to the late Professor C. G. Seligman, F.R.S., President of the Royal Anthropological Institute from 1923 to 1926. As the Institute raises endowment, the Trustees of the Fund are being instructed to place an equivalent amount in the Institute's Endowment Fund. If, at the end of five years, the Institute has not raised its portion of the endowment, the remaining capital may be diverted to some other institution. In the meantime, the Institute will enjoy the interest on the capital.

This exceeding generosity puts the Institute's endowment target of £50,000 within the realms of possibility.

A brief ceremony was held to take leave of the mask, which has been in Mrs. Seligman's home for many years, and she made a few remarks on the occasion. I thought that you might be interested in recording these remarks since they express so aptly the sentiments which have led her to take the step which means so much to the Institute. The toast was 'To the Benin Mask coupled with the Royal Anthropological Institute, affectionately known as "The Anthropol".'

'The mask has had a chequered history. Once part of the Benin royal regalia, it was held in very high esteem. "Collected" or "liberated" in 1897 by Sir Ralph Moor, it later passed to a member of his family, who with painful memories of West Africa as the White Man's Grave, merely wanted to get rid of it in 1909 when Sligs [Professor Seligman] acquired it.

'Its general esteem rose slowly, but recently it has gone up and up. As its value rose, so did the necessary expenses of the Anthropol, while the financial resources went down and down till we are not far off bankruptcy.

'The mask may save us from this. But even the mask cannot provide an endowment fund large enough to allow us to cease practising irritating economies and to let us expand as we should. It is now up to the Institute to double the sum it may acquire from the mask.

'The ivory mask brings me to Ivory Towers. In these days of skyscrapers, they are very much out of fashion, so that one becomes ashamed to talk of "art for art's sake" or "knowledge for the sake of knowledge." Let me point out the utility of the useless. Some non-anthropologists have asked what is the use of the Institute at all? And some anthropologists suggest that the only worthwhile anthropological investigations are those which give economic and other statistics, or describe the changes from tribal to industrial life. I do not want to belittle such studies, but to emphasize that anthropology should be what it says it is—the study of man. Our knowledge of man and his culture is still incomplete. There are still in New Guinea and South America, even in Africa, and probably many other parts of the world, peoples whose culture has developed without direct contact with the great civilizations and who have not yet been studied. It is for the Anthropol to encourage both kinds of studies.

'To return to the mask: it is from the love of art for its own sake that the sale of the mask has come about. The lady who merely wanted to get rid of the mask took it to a dealer in Chinese art. The dealer bought it and another which came with it (now in the British Museum) only because he thought they were so beautiful. Not knowing anything about it, he consulted Sligs. The mask has realized, almost fifty years later, a net sum of £20,000. It will go to the Museum of Primitive Art, New York, where I hope that it will be appreciated as much as it was when it was in Benin, though in a different way, and by a wider public. And I hope that it will stimulate the interest of scholars and art-lovers in the arts of West Africa and the value of anthropology.'

MARIAN W. SMITH

Royal Anthropological Institute

Hon. Secretary

#### Note

Many people, whether Fellows of the Institute or not, would no doubt like to possess a fine reproduction of the Seligman mask, which has thus proved itself the most valuable ethnographical

specimen ever put upon the market. As was announced on the cover of the April issue of MAN, copies are on sale at the Institute in aid of the Endowment Fund, price 5 shillings, of the excellent photograph made by the well-known art photographers, Cowderoy & Moss, Ltd., shortly before the mask was consigned to its new owners.

The Hon. Editor would like to commend most earnestly to all readers of MAN the appeal to which Mrs. Seligman has so signally lent her support in what must surely be the most generous investment ever made by a private person in the future of anthropology. The task of matching her offer is a great and difficult one; all benefactions, from the smallest to the largest, will be valuable and welcome, and readers may be sure that they will benefit MAN, among the other works of the Institute, and may even be essential to its survival as the economic difficulties of publication become more severe.—ED.

Elephants and Ethnologists. Cf. MAN, 1958, 9

**125** SIR,—In his review of *Die Mayaforschung* Mr. Braunscholtz speaks of 'Waldeck's wholly apocryphal elephants' heads and trunks,' and goes on: 'he could hardly have foreseen that those same drawings would be used nearly a century later by Elliot Smith as conclusive evidence of Asiatic influence!'

In Elliot Smith's *Elephants and Ethnologists*, on p. 14, we read: 'Alfred P. Maudslay, *Biologia Centrali-Americana* (Archæology 1889–1902), Part II, text, November, 1900, p. 43, Plates XXXIII to XXXIX. This classical monograph provides the evidence from Copan upon which my argument is based. Although Dr. Maudslay emphasizes the Asiatic features in the design upon Stela B, and refers to the elephant-like appearance of the animals with trunks, he somewhat inconsistently expresses the view that they are conventionalized tapirs.' There follow a number of plates and figures described as being 'after Maudslay,' and other figures of alleged American elephants are described, with references, as being 'after Selser' and 'after Tylor.' There is no mention of Waldeck, and it is difficult to believe that Elliot Smith copied his drawings and then attributed them to Maudslay and others.

Usk, Monmouthshire

RAGLAN

#### Note

Lord Raglan's letter has been shown to Mr. Braunscholtz, who writes as follows.—ED.

Lord Raglan is quite correct in saying that there is no mention of Waldeck in *Elephants and Ethnologists*. At the date of its publication in 1924 Elliot Smith does not appear to have known of Waldeck's drawings. The passage in my review about Waldeck's 'apocryphal elephants' referred (though I did not cite my reference) to the publication by Elliot Smith of a lengthy article in *The Times* (14 January, 1927) and another in the *Illustrated London News* (15 January, 1927) containing illustrations of water-colour drawings of elephants' heads by Waldeck. These drawings, discovered by Mr. Eric Thompson in the Newberry Library at Chicago in 1926, had been submitted to Elliot Smith, who promptly used them as the final and decisive proof, which 'ought to settle the elephant controversy once for all.' Unfortunately he does not appear to have examined Waldeck's credentials as a reliable witness, and even went so far as to say that 'no-one is likely to doubt the accuracy of the representations of elephants' heads.' But, as I pointed out in a short letter to *The Times* a few days later, the accuracy of Waldeck's proboscidean drawings is not merely open to the gravest doubt; his drawings of elephants' heads from the Temple of Inscriptions at Palenque, published in *Monuments Anciens du Mexique*, etc. (edited by Brasseur de Bourbourg, 1866), on Plate XXXVIII, are wholly fictitious, as anyone can verify by comparing them with A. P. Maudslay's photographs and meticulous drawings of the same subjects in *Biologia Centrali-Americana* (Archæology), Vol. IV., Palenque, Plates LVIII and LXI, glyphs G4, H3, and J2.

Waldeck's penchant for elephants had indeed been critically



commented on many years earlier by C. J. D. Charnay in *Les Anciennes Villes du Nouveau Monde* (1885), p. 209. The passage is perhaps worth quoting: 'C'est au milieu des katunes de cette dernière inscription, qu'il a plu à Waldeck de nous dessiner trois ou quatre têtes d'éléphants avec leurs trompes, leurs petits yeux, et leurs grandes oreilles. Dans quel but ce voyageur faussait-il ainsi l'histoire? Dans le but de présenter ces ruines comme de véritables fossiles, puisqu'il est bien établi que l'éléphant n'a jamais été connu en Amérique qu'à l'état fossile. Il devenait alors tout naturel d'attribuer aux constructeurs de ces palais une antiquité des plus reculees, presque fabuleuse, puisqu'ils avaient connu ces pachydermes. Inutiles d'ajouter que ni Catherwood, qui dessina ces inscriptions avec le soin le plus méticuleux, ni nous qui en avons rapporté des estampages, ni personne au monde n'aperçut jamais ces éléphants ni leurs trompes. Voilà où conduisent le parti pris et l'amour du merveilleux.'

Although none of Waldeck's drawings figure in *Elephants and Ethnologists*, that work does contain other examples of the use of inferior and misleading illustrative material, e.g. a drawing, taken from A. von Humboldt's *Vue des Cordillères* (second edition), of a figure from the Mexican *Codex Borgia*, in which a speech sign has been distorted to look like an elephant's trunk. Such examples are a warning against the use of unverified copies in cases where, in default of the originals, facsimiles or photographs are available.

I must apologize for trespassing thus far on your valuable space; but I am grateful to Lord Raglan for giving me an excuse for attempting, 'once for all,' to lay the ghosts of Waldeck's elephants, which may still be skulking in obscure corners, ready to raise their chimerical heads once more to the confusion of archaeological truth.

**Ashanti and Hebrew Shamanism.** Cf. MAN, 1958, 7

**126** SIR.—In Dr. M. J. Field's letter recently published in MAN, to support a construction that Jesus did not die on the cross, she quotes the statement that Pilate 'marvelled if he were already dead,' without making any allusion to the immediate sequel. For the sake of scientific accuracy, though it be at the cost of taking the sand from under Dr. Field's foundations, let me complete the quotation:

'And Pilate marvelled if he were already dead: and calling unto him the centurion, he asked him whether he had been any while dead. And when he knew it of the centurion, he gave the body to Joseph.'

London, S.W.3

A. R. H. MACDONALD

**Anthropomorphic Crucifixes in Sinai.** Cf. MAN, 1947, 141

**127** SIR.—Under the above title you were good enough to print, in September, 1947, an illustrated description by me of certain cruciform images which had been deposited on an ancient tomb at Awlad 'Ali beside the Wadi el-'Arish in Sinai. My article ended with the words: 'The intention to make them as human as possible was clearly evident. But as to why the images were deposited there (annually) the shy Tarabin would not vouchsafe any explanation.' Upon a later visit in August, 1951, I was able to obtain more information. These brightly dressed images are called by the Tarabin 'brides' and are awarded as prizes to the successful competitors in the horse and camel races held annually on the spot. My Muslim informant was rather ashamed of such pagan goings-on.

Milltimber, Aberdeenshire

G. W. MURRAY

**Attrition of the Teeth among Tibetans.** Cf. MAN, 1957, 227; 1958, 44, 45, 92

**128** SIR.—With reference to the letter of Professor C. F. Ballard and Mr. J. R. E. Mills of the Institute of Dental Surgery concerning my article on 'Attrition of the Teeth among Tibetans,' while I cannot comment, for want of professional knowledge, upon their assertion that the lack of attrition among certain races of man is abnormal, I should like to make the two following remarks:

(i) I do not think that Western European society presents more 'extensive intermarriage between different racial groups' than the Tibetan. From my anthropometrical measurements of Tibetans, I gathered the impression that they constitute as much of a mixed race as we do, if not more so. The final results of my work are unfortunately not yet available, but they are being at present prepared in Copenhagen and I believe that when published they will bear me out on this point.

(ii) I do not understand the sentence 'to use the teeth as evolution originally determined.' Is not evolution a process that is never arrested, which is going on all the time, even to the point of creating today malocclusion in individuals, who, despite it, survive in our society in ever increasing numbers? I cannot bring myself to consider 'Evolution' as a synonym of 'Providence,' which is, surprisingly enough, what the writers appear to be doing.

London

PETER, Prince of Greece and Denmark

**'The "Eskimo Ulu" in the Malayan Neolithic.'** Cf. MAN, 1958,

**129** 38, 91  
SIR.—The title of article 38 in your March issue is, to say the least, unfortunate, even though the words 'Eskimo Ulu' are placed within inverted commas. It is only fair to the author to point out that he does not claim an Eskimo origin for the stone blades found in Malaya but only that the *ulu* and the Malayan knives may have a common ancestry in the Neolithic of Southern Siberia.

G. H. S. BUSHNELL  
University Museum of Archaeology and Ethnology, Cambridge

#### Note

The arresting title and its punctuation were the author's.

(With regard to the use of inverted commas, this may be a suitable occasion to state for the benefit of contributors that the invariable practice of MAN in this matter conforms with that of most of the best British printers, and with the recommendation of Fowler: 'The more sensible practice is to regard the single as the normal, and to resort to the double only when, as fairly often happens, an interior quotation is necessary in the middle of a passage that is itself quoted' (*Modern English Usage*, 1926, p. 570, s.v. 'Stops: Inverted Commas')—a practice aptly illustrated by a comparison of the title of Prince John Loewenstein's article with its quotation at the head of this letter).—ED.

**The National Museum of Southern Rhodesia: A Correction.**

Cf. MAN, 1958, 40  
**130** Mr. Roger Summers writes to point out that he is not, as stated in Mr. E. C. Lanning's article, Curator of the National Museum of Southern Rhodesia at Salisbury, but Keeper of Antiquities at the National Museum of Southern Rhodesia, Bulawayo.

## REVIEWS

### ASIA

**Les Populations du Cambodge.** By Georges Olivier. Paris (Masson), 1956. Pp. 164. Price 1,600 francs

**131** Although there have been a number of investigations relating to the physical anthropology of Indochina, these have for the most part referred to the peoples of north and east—the Vietnamese and the mountain groups of the

Chinese frontier. Dr. Olivier's study, predominantly anthropometric, is an attempt to remedy the deficiency for the south-west. He had hoped to make a survey covering all the populations of Cambodia; the state of the territory rendered access to the primitives out of the question, and he had to be content with one sample from each of the major populations and from others in Vietnam.



The bulk of the population of Cambodia is composed of the Khmers, some three and a half million in number (excluding the colonies in adjacent territories), possessing common language and customs and fairly homogeneous in appearance, the descendants of the builders of the Khmer empire. Olivier measured 440 adults in a number of centres, avoiding the sparsely settled east and the cosmopolitan Phnompenh, and examined the ABO blood groups of 500. Culturally distinct are the 70,000 Chams, differing in language, custom and religion, descended from refugees from the Cham empire of the Annam coast on its collapse under attacks from Tonkin; a sample of 92 resident in two villages near Phnompenh was measured. The third group of peoples, the autochthonous primitives of confused nomenclature, inhabits the more remote and mountainous regions; to represent these a sample of 30 Mois from Vietnam was measured, considered to be related to the Pnong of eastern Cambodia. Finally for comparison measurements were obtained on 50 Vietnamese. On most subjects 7 metrical and 13 qualitative characters were recorded, but for 50 Khmers, 30 Mois and 50 Vietnamese fairly complete body measurements were obtained. The most comprehensive anthropometric investigation in Cambodia to date, the results are extensively presented with a wealth of comparative data for the region from the literature, and the volume therefore serves as a useful sourcebook. Points of interest among the results are that the stature of adult Khmers does not appear to have changed over the last 50 years; that the cultural distinctness of the Chams is supported by their differences in metrical features and ABO blood-group frequencies, the latter being similar to those of the Vietnamese; and that there is considerable heterogeneity among the primitive groups.

D. F. ROBERTS

**Ancient and Modern Man in South-West Asia.** By Henry Field. Miami, Florida (U.P.), 1956. Pp. 342

**I32** This is a valuable compendium of knowledge of ancient and modern types of man with much tabular matter and little stress on conclusions. Extracting data from the tables we find that groups including 1954, mostly Beduin, in Egypt have a mean cephalic index below 74, 3850 a mean cephalic index between 74 and 76, 1713 from the Canal Zone and the adjoining province of Sharqiya have the mean 76-77, and only two small groups of fellahin have it a little higher. Of 17 crania from Sinai, 14 have the cranial index below 72, with one each at 73, 74.2 and 77.6. All this, with the study of ancient skulls, supports the long-held view of South-West Asia and North-East Africa as a most important home of early hyperdolichocephalic *Homo sapiens*. Many gradations from the rough to the gracile type are found. The bibliographies for each region will be found invaluable by investigators. The book bears throughout the marks of Dr. Field's many years of careful and devoted work and travel and gives full appreciation of the work of many other men and women.

H. J. FLEURE

**Twenty-Five Years of Mesopotamian Discovery—1932-1956.**

By M. E. L. Mallowan. London (Brit. Sch. of Archaeol. in Iraq), 1957. Pp. 80. Price 5s.

**I33** This booklet has been produced to commemorate the Silver Jubilee of the British School in Iraq, and to accompany the exhibition which was arranged for the occasion in the British Museum. It sets out with admirable conciseness and clarity the history of the School's work, from the beginnings at Arpachiyah in 1932, through the excavations in the Khabur valley and the soundings by the Balikh, to the post-war series of seasons at Layard's Nimrud. Mr. Mallowan explains why each enterprise was undertaken, what results it produced, and what problems it has left. This makes an extremely neat and valuable reference volume. But it also serves to recall the special virtues of the British School in Iraq. First, its work has always been published in full detail and with exemplary promptitude. Secondly, its strategy has been planned with extreme care, concentration and foresight, to avoid duplication of effort and distraction by side issues, however attractive. It must have taken great effort of will, for example, to pass by the Roman remains in the valley of the Balikh. A list of a few of the more outstanding discoveries of the School—the Arpachiyah painted bowls and *tholoi*,

the Khabur and Nuzi wares, the Brak eye temples, the Nimrud ivories and the Assur-nasir-pal inscription—is in itself testimony to the range and importance of its achievements over a period which, with the war interval, amounts to only 15 active years. This pamphlet is a modest but triumphant vindication of the ideal of Gertrude Bell and of the efforts of all those who have given and are giving their support and energies towards its fulfilment.

W. C. BRICE

**The Disintegrating Village: A Socio-Economic Survey Conducted by the University of Ceylon, Part I.**

**I34** By N. K. Sarkar and S. J. Tambiah. Colombo (Ceylon U. P.), 1957. Pp. xvi, 83.

In the summer of 1955 certain of the staff and students of the Departments of Economics and Sociology at the University of Ceylon carried out a questionnaire sample survey of the whole population of Pata Dumbura District, a region containing 58 villages and 17,561 households. The investigating team numbered 31 of whom only two were fully qualified social scientists. The period of enquiry covered 10 days.

The utility of such an investigation clearly depends upon a variety of factors, notably the nature of the facts under investigation, the kind of knowledge of the facts already possessed by the planners of the survey, and the skill with which the sample and the questionnaire is designed so as to overcome the limitations of time and investigator quality.

That part of the enquiry which is analysed here was mainly concerned with such matters as property ownership, inheritance, crop yields, and sources of income, and the planning of the survey was clearly most carefully and skilfully performed. On the other hand the authors at times appear rather naive in the reliance which they are prepared to place upon the resultant figures.

Some formidable statistical analyses have been applied to the numerical results of the questionnaire investigation, and some of the conclusions which emerge are highly interesting, yet it is difficult to know just what has been demonstrated. An anthropologist is naturally sceptical of short-run enquiries, and the field of economic relationships is certainly not one in which he would ordinarily expect candid answers from randomly selected informants. It would seem that survey work of this kind has only limited value unless it can be supplemented and 'validated' by more detailed local investigations such as those in which the social anthropologist ordinarily specializes.

In fact however the present work does not stand by itself. Another section of the results of the same investigations is shortly to be published in the *Journal of the Royal Anthropological Institute*. In this latter case the subject matter is kinship rather than economics and the author (S. J. Tambiah) relies on case histories rather than generalized statistics to demonstrate his thesis.

The two documents taken together thus provoke some interesting questions concerning the kind of 'truths' which sociologists and social anthropologists are alike seeking and the best method of reaching them. From this point of view the volume under review is of greater general interest than its specialized subject matter might suggest.

In the field of Ceylon social studies the survey is an important landmark and the authors are to be congratulated on their enterprise in organizing and completing their research. They also deserve warm praise for the prompt publication of their findings.

E. R. LEACH

**Chinese Family and Marriage in Singapore.** By Maurice Freedman. Col. Res. Stud., No. 20. London (H.M.S.O.), 1957. Pp. 249, plates, tables, diagrams. Price £1 10s.

**I35** This report is based on field experience in rural and urban Singapore. Dr. Freedman worked largely among Hokkien Chinese, the predominating group in Singapore, and he speaks their dialect. He is concerned with the role of kinship in the ordering of Chinese social life in Singapore today; the extent to which the family pattern of China is reproduced overseas and whether the family helps or hinders adjustment to new economic and political conditions overseas.

The report contains an introductory chapter dealing with the



home background, reasons for immigration and typical methods. This is followed by a discussion of the household, its numerical size and composition. A move away from economic unity of the household in production is noted, particularly for urban Singapore.

In discussing kin with which Chinese are not domestically involved, Dr. Freedman says that overseas representatives of a particular lineage have not normally grouped themselves in a manner reproducing the home organization on a smaller scale; the ancestral village is still considered as the lineage seat. Local kindred are not territorially compact or persisting exclusive groups with corporate personalities. There is also a shift in emphasis away from the patrilineal bias of home kindred. A comprehensive account of kinship terms of reference and address is given using romanized Hokkien. Character equivalents for these terms and other Hokkien expressions in the text are given at the end of the report.

Further chapters deal with the structure of marriage, its social area, polygamy, marriage disrupted and agencies of reconciliation. An interesting example of how Chinese society in Singapore has moved away from conformity to patrilineal organization is in the way in which sons are now often handed over to mothers when there is a divorce, thus passing out of control of the patrikin. There are two chapters of considerable detail. One on the formation of marriage includes descriptions of modern forms; the other on death and its cult discusses the role of associations in the care of the dead.

Summing up, Dr. Freedman says that the agnatic principle as it survives in Singapore has become the basis for association activities and defining loyalties between persons of identical surname rather than a method for bringing corporate groups of patrilineal kinsmen together. The household is the only kinship unit with clearly defined boundaries and activities. There has been a change in the relative status of primary and secondary wives and a shading of polygamy into mistress-keeping. A need exists for clearer formulation of the legal status of Chinese marriage and divorce.

This report is intended primarily for non-anthropologists. Much of its terminology might be confusing, however, to those without some basic knowledge of social anthropology. Dr. Freedman deals with a number of important questions in an able and comprehensive way and his conclusions should be of value and interest to all those concerned with the problems of plural societies and of social change.

MARJORIE TOPLEY

### Indonesian Society in Transition: A Study of Social Change.

**136** By W. F. Wertheim. Issued under the auspices of The Institute of Pacific Relations. The Hague and Bandung (Van Hoeve), 1956. Pp. xiv, 360. Price 15 florins

In this book Dr. Wertheim has a two-fold target. Political independence has brought modern Indonesia into new relations with the outer world and has created more problems than it has solved. He aims at helping the leaders of the struggle for independence towards a clearer insight into these new problems, and also at encouraging in the outside world a sympathetic understanding of Indonesian reactions to these problems so that friendly overtures may be more acceptable and fruitful. Accordingly his main purpose is to elucidate the essential character of modern Indonesian society. For this task he is well qualified by personal experience in Indonesia and by his special studies of history and sociology. The first four chapters deal briefly with the geographical and historical background of the nationalist movement. Then in successive chapters he analyses modern developments in the economic system, the social system, urban life, religious trends and labour problems. Under each head he looks at Indonesia before it felt the impact of the West, and then traces more closely the course of events in the nineteenth century and the early years of the present century. This clears the way for a stimulating survey of the Japanese occupation and its consequences, as an introduction to the tangled story of the birth pangs of national independence. In the concluding chapters he discusses cultural dynamics and the reactions of independence on the nationalist movement. A postscript carries on the story until the end of 1955. 'At the moment the picture conveyed by the state of Indonesian culture is largely one of disintegration.' As in other parts of South-East Asia, political independence has created the fundamental problem of reintegration in a national society.

The present leaders were carried forward on the floodtide of nationalism. With the attainment of independence, the tide has receded, leaving them stranded on the shore. They do not quite know what to do next. Some of them are satisfied with their position; they have lost their nationalist fervour and do not very much want to do anything. Yet they cannot stay where they are, for they are on a quicksand which will engulf them unless they find solutions for the problems that threaten their security. Wide seas still separate the islands, inhabited by people of diverse racial origin and in some cases of very different cultural levels, who were united only in their common opposition to foreign rule, whether Japanese or Dutch. New centrifugal forces are becoming manifest. Islam is a binding force, but also a stumbling block, separating those who seek a spiritual home in an Islamic State from those who have more faith in modern progress. Progressives are divided in their attitude to America and Russia, fearing and admiring both. Even those who think they know what to do, do not know how to do it; for foreign rule has left them without practical experience of leadership in government and business. All this breeds a frustration and dissatisfaction with democracy that may end in military dictatorship.

Yet Dr. Wertheim regards nationalism as still 'a major spiritual force in free Indonesia.' He looks forward to the future with a cautious optimism. What, however 'is required is a change in the whole economic structure.' The western world might help Indonesians, if they would accept its help. 'Such help, however, would not only imply a revolutionary change of mind in the West, but also little short of a revolution of its economic and social system.' But this book is testimony to his faith in Indonesia, even if his faith rests on the evidence of things unseen. It certainly deserves careful study by all those in the western world who aspire to help Indonesia and other 'under-developed countries.'

J. S. FURNIVALL

### Eastern and Western Worlds. Selected readings with a foreword by S. Hofstra. The Hague and Bandung, 1953. Pp. 181

**137**

This volume comprises the texts of 11 lectures delivered by Asian and western scholars at the Summer Sessions of the Netherlands Universities held at Leiden in 1951 and 1952. They cover such topics as the meeting of East and West, the social position of women, population increase, the post-war resurgence of Japan; and their content ranges from generalities which hardly challenge dissent to more particular studies of matters in either the east or the west. There is little fundamental unity among them, most suffer from the shallowness of short non-specialist lectures, and the majority are of little specific concern to the anthropologist; but two stand out on their own merits (though, given the non-technical character of the lectures, this is largely a personal selection), regardless of their relevance to particular disciplines. Professor Logemann gives a thorough survey of the characteristics of urban centres in the East and the grave social problems accompanying their unsettling and extraordinarily rapid growth. Professor Duyvendak discourses in a masterly and engrossing way on the Chinese influence on European thought, particularly in the eighteenth century; claiming at one point, for example, that the discovery of Chinese civilization powerfully aided the development of critical thinking in Europe and even assisted in preparing the attitude towards state and church which finally led to the French Revolution.

Professor Hofstra points to the practical unity of the modern world and to the human and political need to study the complex of issues subsumed under the misleadingly antithetical catchwords 'East' and 'West.' He urges the need for greater knowledge and understanding, to which the social sciences should contribute, and hopes that the lectures may help to arouse human sympathy. High aims and worthy hopes, expressed by a number of the contributors also; but the depressing reality in this bad world (as notably Ruth Benedict never saw in her own comparison of other cultures and their peculiar orientations) seems to be that greater knowledge may as well lead to detestation as to sympathy, that values cannot be reconciled by the most sympathetic recognition of differences, and that the courses of power are not directed by compassion.

RODNEY NEEDHAM



**Hierarchy and Marriage Alliance in South Indian Kinship.** ByLouis Dumont. *R. Anthropol. Inst. Occ. Pap. No. 12.* London (R.A.I.), 1957. Pp. 45. Price 10s.

138

Dr. Louis Dumont's study of hierarchy and marriage alliance in south Indian kinship is a most valuable contribution to the knowledge of Indian social structure. By means of a comparative examination of five Tamil caste groups, component sections of three different castes, he has convincingly demonstrated that the kinship pattern common to these groups, in which descent, inheritance, residence (matrilocal or patrilocal, that is), and marriage rules vary considerably, depends on a complementary duality of 'kin' and 'alliance,' unilineal descent being complemented by alliance made under the form of a particular pattern of marriage rules and of ceremonial gifts and functions. The principle of descent and other features may vary greatly but the fundamental principle of 'alliance' varies little and is probably common to South India in general. He arrives at this conclusion by a meticulously careful and penetrating analysis of the societies examined and illustrates his examination with lucid diagrams.

Incidentally Dr. Dumont demonstrates very aptly the anomalous hierarchy of endogamous or partially endogamous sub-groups within the endogamous caste, a phenomenon which probably characterizes in a greater or less degree the majority of Indian caste groups, and rightly observes that this may throw light on the alleged origin of castes from *varna* as described by Manu. The author's list of references and his use of them is testimony to his careful and thorough scholarship. One could wish that someone would examine with similarly scrupulous and exhaustive care the difficult question of Right and Left Hand castes in southern India.

J. H. HUTTON

**North Borneo, Brunei, Sarawak (British Borneo).** *Human Relations Area Files, New Haven, 1956. Pp. xi, 287.*

139

Price \$7

This book, according to the preface, is the second volume of a Country Survey Series on certain areas and peoples of Eurasia and North Africa to be prepared by a group of American scholars. For the sake of comparisons a standard form is to be followed in the volumes of the series; and in this one an attempt is made to arrange all the available information on North Borneo, Brunei and Sarawak in a systematic way.

For a book presumably aimed at readers without specialized interests in sociology, geography, or economics the headings under which the information is assembled (for the most part only from published sources) are sensible; and the summaries of the societies and the general conditions in the area are adequate for a handbook.

The editors are well aware of the shortcomings of a compilation of this kind, and the inconvenience of having the information about any one society scattered throughout the book probably could not be avoided. Rather more serious are the disadvantages which arise from employing several writers who necessarily use the same material, but whose interests and assumptions differ. On the whole the editor has succeeded in eliminating contradictions of fact and opinion, though the reader will perhaps be surprised at being told on p. 82 that the powers of the village headman 'among groups with more aristocratic traditions, such as the Iban, may be stronger' when he has already been informed on p. 43 that the Iban are 'at once competitive and equalitarian.'

In the preface the editor tells us that 'British Borneo, like so many of the under-developed countries, is both colonial and plural in its

socio-economic structure.' Behind this unexceptionable statement and much of what is said elsewhere in the book lie implicit assumptions about the nature of colonial and plural societies and their welfare. These views, which are not discussed, gain strength by calling the area British Borneo and treating it as if it in fact possessed some kind of unity beyond the responsibility of the administrations to the Colonial Office. The topic, however, would perhaps have needed more careful consideration than was appropriate in a handbook of this kind.

H. S. MORRIS

**Sarawak.** By Hedda Morrison. London (Macgibbon & Kee), 1957. Pp. 288. Price £3 3s.

140

For those with an interest in Borneo or South-East Asia generally this is a delightful book. Mrs. Morrison is the wife of an official in Sarawak; and for a period of eight years she has travelled extensively in a country where travel is not easy. Her photographs are well known in the Far East and in this book she has made a selection of them for publication in the form of an album. Her enjoyment of her travels and her liking for the people of Sarawak are evident on every page.

She has grouped the photographs under the heads relating first to the scenery and the economic life of the country. Then follow sections dealing with the various peoples of the country; and the photographs give one, without even reading the letterpress, an excellent idea of what the people look like, how they live and gain a living, and what they do to amuse themselves. The letterpress is unobtrusive and sensible, though as in similar books which display the English countryside and omit the more squalid industrial towns, Mrs. Morrison has restricted herself in the main to the traditional and more picturesque aspects of Sarawak. In the circumstances nobody is going to quarrel with her for doing this. Indeed one can only regret that generous patrons such as the Royal Dutch-Shell Group, who made this book possible, and enthusiasts like Mrs. Morrison do not often meet so happily in other parts of the world.

H. S. MORRIS

**Die Sichule-Sprache auf der Insel Simalur an der Westküste von Sumatra.** By Hans Kähler. *Afrika und Übersee, Supplement 27.* Berlin (Reimer), 1955. Pp. 75. Price DM 10.60

141

Our knowledge of the languages of the main island of Sumatra has not made much progress during the last decades, because of its size and the large number of languages spoken; and the Second World War put a stop to nearly all research. But as for the tiny islands on both sides of it, we have learnt a great deal thanks to the numerous studies which Dr. H. Kähler of Hamburg has contributed to *Anthropos* and other publications during the past 20 years.

In this grammar the author, who is a pupil of O. Dempwolff, uses the framework of his Malay Grammar (*Zeits. f. Eg. Spr.*, Supplement 22, Berlin, 1941) for the description of this language which he studied *in situ* in 1938. It was at that time spoken by only 3600 people, and showed substantial influences from the languages of Nias and Simalur, as well as from Atjeh and Minangkabau; nowadays the unifying Bahasa Indonesia will be the strongest influence upon this dying language. The author noted down a considerable number of as yet unpublished texts and also made a dictionary which has not yet appeared in print; it is to be hoped that these scholarly works will be made accessible in due course.

C. HOOYKAAS

## EUROPE

**Bauernwerk in Italien, der italienischen und rätomanischen Schweiz, Vol. II.** By P. Scheuermeier. Bern (Stämpfli), 1956. Pp. xiii, 529, 495 woodcuts and drawings by P. Boesch, 542 photographs, 13 distribution maps. Price

142

70.45 Swiss francs

The second volume, illustrating rural life in Italy and Southern Switzerland, although a work in its own right, is the latest addition

to the monumental *Sprach- und Sachatlas Italiens und der Südschweiz* by K. Jäger and J. Jud. Scheuermeier's laborious preparation covered almost 40 years. He travelled twice from the Alps to Sicily, enquiring at innumerable farms after the local names for material objects, the work done by the farmer and his wife, social conditions and traditional customs. Having set out as a linguist, the author became an expert on material culture. During his second journey,



Scheuermeier was accompanied by P. Boesch, an excellent draughtsman, whose neat drawings and woodcuts are a perfect match to the concise text.

Herr Scheuermeier's undertaking was timely. It saved from oblivion many an object which went out of use during the last two decades and which was destroyed during the war. In view of the considerable timelag between collecting and publishing, the timetable of the photographs (pp. 485f.) is of decisive importance. Anthropologists and archaeologists alike will welcome the chapters on primitive and archaic buildings, the development of the hearth, pottery, means of transport and carrying loads, and primitive methods of baking, etc. Folklorists will greatly profit by the photographs of festivals, traditional costumes and rural craftsmen, as well as by the chapters on the laundry (an important item which has often been disregarded), hemp, flax, wool, spinning and weaving.

Preparing a journey through the Grisons eight years ago, I spent several afternoons at the Rhaeto-Romanisches Institut at Samedan, extracting from the voluminous *Sprach- und Sachatlas* copious notes, which later proved indispensable. May I plead here on behalf of less leisured travellers for a shortened version which can be easily consulted *en route*? Perhaps it will include some brief introductory remarks about the influence exercised by the German-speaking neighbouring provinces and countries, very obvious in quite a number of photographs.

E. ETTLINGER

**Social Relations in a Nomadic Lappish Community.** By Ian Whitaker. *Samiske Samlinger II*. Oslo (Norsk Folkemuseum), 1955. Pp. 178

143

This book will be welcomed by all those interested in Lappish culture and society, for it represents the first publication on Lapp social relations based upon the systematic field studies of a social anthropologist. The efforts of Dr. Ian Whitaker and certain others—Robert Paine from Oxford, Ralph Bulmer from Cambridge and the late Dr. Robert Niel Pehrson from Chicago—have been pioneer in this particular branch of Lappish studies. Ian Whitaker chose to gather the material for his book from among the reindeer-breeding Lapps of Lainiovuoma in the southerly part of the parish of Karesuando in North Sweden.

The monograph is not a large one. Of 178 pages from cover to cover, there are 113 pages of main text divided between 15 short chapters (the last of which is no more than half a page).

In his introductory chapter, the author states that 'the principal unit' in Lappish social organization, the *sii'da*, has been omitted, almost without exception, from previous descriptions of Lappish life. In the author's own bibliography one finds Erik Solem's *Lappiske Rettstudier*, where (on pp. 81-4 and 184-8) Lappish social organization and the Lappish concept of *sii'da* are both considered, and V. Tanner's *Skoltlapparna*, where (on pp. 388-404) a summary is made of the references to *sii'da* in earlier writings.

Chapter 2 provides geographical and historical outlines. Here we notice, in particular, that all the geographical data are squeezed onto one of the nine pages of the chapter, and certain of the names on a map are unhappily placed.

In Chapter 3 the reindeer-herders' cycle of activities and economy are handled. It is here that one might expect to find the main body of the field material, especially since—as we are told in the editor's Preface and again in the author's Introduction (pp. 11f.)—the author himself took an active part in reindeer-herding activities while in the field, and also especially since the author believes 'the principal force [in the society] to be a contiguity of economic interest: the demands of reindeer husbandry' (p. 15, my italics). The chapter is of ten pages.

The law concerning the right to own and herd reindeer is stated, and some attention is given to the mountain reindeer's wanderings between the winter and summer pasture areas.

The fourth chapter is devoted to the family and the obligations of kinship, but in the fifth and the sixth it is again reindeer-herding—the organization of herding—that is considered. Here too, one is left wishing that more of the author's own field material had been presented. Chapter 6 consists mainly of references to the Norwegian-Swedish reindeer commission and of the summer pasture districts used by certain Swedish Lapps in Norway.

Chapter 7 has the title 'The economic modification of society.' Again, the documentation from Lainiovuoma has been overshadowed by theoretical consideration mainly constructed from data relating to other groups of Lapps and other arctic peoples. Property and inheritance are handled in the next chapter; the treatment of the delicate matter of reindeer-stealing (about which the author admits that he found it difficult to collect material) is unsystematic. At all events, after reading the chapter one must agree with the author that this is an institution deserving further attention.

In Chapter 9, on authority and group solidarity, a good deal of useful data from Lainiovuoma is provided, especially concerning labour and the conditions of employment of hired labour.

The discussion in Chapters 10 to 14 is directed to the relations that the Lapps of Lainiovuoma maintain with other Lappish groups and with peasant groups in both Sweden and Norway. Once again, it is regrettable that more of the field material has not been brought forward. In the five pages of Chapter 14, for example, only about three lines relate to the situation in Lainiovuoma. In the remainder of the chapter the author attempts to outline the different historical phases through which attitudes held towards Lapps have passed. This is, of course, too complicated a topic to be solved in the space of these few pages, and it is therefore not surprising that this contribution fails to bring order out of the chaos of misunderstandings and faulty conclusions that permeates both the contact situation itself and the debate to which it has given rise.

The book as a whole has two main shortcomings: on the one hand, the author has tended to over-generalize from his material—which is not as rich as might have been expected—and on the other, his too frequent use of reference and citation has drowned the Lainiovuoma material in a welter of material from other areas.

These objections notwithstanding, this monograph from Lainiovuoma will be assured the interest of all those working in Lappish studies. The author is to be congratulated on his initiative, and we may surely expect further original field material from him?

ÖRNULV VORREN

**Irish Folk Ways.** By E. Estyn Evans. London (Routledge & Kegan Paul), 1957. Pp. 324, xvi, illus. Price £1 15s.

144

Fifteen years ago in *Irish Heritage* Professor Estyn Evans gave us a foretaste of his new work on Irish folk culture. The present book is a much fuller development of the subject and a valuable addition to the literature.

The Irish peasant has shown a greater resistance to the changes wrought by progress than his counterparts in the other Celtic lands. This is due to a number of reasons, but foremost among them are the poverty of the country, the sad course of Irish history and the fact that Ireland has remained a strong agricultural community; for example, in 1840 less than 15 per cent. of the population were urban. As a result Ireland is a happy hunting ground for the ethnographer. Professor Evans's book provides much material and will help to make clear the origins of practices and techniques once found in other parts of the British Isles. There must always be borne in mind, however, perhaps more strongly than Professor Evans suggests, the widely divergent social systems of Celtic and Anglo-Saxon settlers. It is not always as reasonable as it may appear to translate Irish intelligence into English terms. Nevertheless, there is a strong bond between practices common in Ireland today, or in the immediate past, and many which were found in mediæval England.

The book follows a pattern chapter by chapter of discussing the methods and utensils used in different departments of everyday life—hearth and home, pots and pans, lazy-beds, wrack and wreck and so on. This is a work of detail more than generalization. Each chapter contains an abundance of carefully collected information supported by Professor Evans's excellent drawings. Currachs, kelp, booley houses, bogs, fairies, slide cars, wakes and poteen are all there, all the ingredients in fact of an Irish stew which threaded together form a fascinating picture of Irish life. The author has been collecting material for some years and it is clear that he started none too soon, for although he writes mostly in the present some of the things he describes are already in the past.

One small criticism that might be made is that Professor Evans does not appear to defer sufficiently to the reader unacquainted with



Ireland and Irish history. More comment on the various cultural influences on and within the island together with a clearer picture of its geography would have been helpful.

It can only be a matter of time before industrialization and the demand for higher standards of living change completely the way of life of the Irish peasant. We can only regret that information of a similar kind to that which Professor Evans has compiled exists for so few parts of the British Isles, but we may be stimulated by this excellent book to do all that is possible in the coming years to ensure that no opportunity is lost to prepare similar records for posterity.

JOHN HIGGS

**Denmark Before the Vikings.** By Ole Klindt-Jensen. London (Thames & Hudson), 1957. Pp. 212, plates, text figs. Price £1 1s.

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This book is one of a series 'Ancient Peoples and Places,' edited by Dr. Glyn Daniel, designed to give a general account of some of the earlier inhabitants of the world. The particular field covered here is that of Denmark in the prehistoric period, when the evidence is purely archaeological as distinct from the written record.

The author covers, in time, just over ten thousand years, yet manages to give the reader a clear picture of the various cultures that he discusses and in particular the development of one from another and the various factors influencing this evolution. Relationship of people and environment is brought out throughout the book. Descriptions of the kind of houses lived in, burial customs and ritual, and the type of farming adopted are all given place, and the way in which the archaeological evidence is used to fill in the picture is well handled. Chronological periods and their succession are always clearly defined. A particular instance of this is the explanation of the decline in trade at the end of the Bronze Age which meant that bronze was less readily available as an import. The metal-smiths had recourse to other raw materials, marking the beginnings of the Iron Age. In a general book of this nature detailed discussion is impossible but sufficient description of significant finds is included for the layman to feel familiar with the various cultures. This familiarity is helped by a set of excellent photographs, striking both in quality, as instanced by the flint dagger, and in arrangement, as seen in the plate of the Dalshøj hoard. Line drawings are included in the text and there is a chronological table as frontispiece. A chapter on the prehistoric landscape contains a map of the chief findspots.

A section on the Danish law concerning Treasure Trove, compensation for finders of antiquities and the general policy of the National Museum completes the book, which has a general bibliography. This is a useful work for readers in this country and credit is due to the translators; one is seldom aware that the book was not originally written in English.

JEAN M. COOK

**Sunken Cities: Some Legends of the Coast and Lakes of Wales.** By F. J. North. Cardiff (U. of Wales P.), 1957. Pp. 256, 14 plates, 17 figs. Price 18s.

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Examining with meticulous care the evidence provided by maps and sea charts, early manuscripts and later records, submerged forests, glacial deposits and other physical features of the coast, Dr. North rejects the references in Welsh legends to catastrophic inundations at the beginning of the Christian era. The story of Llys Helig in Conway Bay is dismissed as an import from Cardigan Bay in comparatively recent times. If the stories of submerged lands in Cardigan Bay are based on a fairly substantial encroachment by the sea, it must have taken place between neolithic and Iron Age times. Early man travelled extensively in that region and a simple tale may have lingered on among the country folk until it was committed to writing in the thirteenth century, just as the story of Caer Aranrhod in Caernarvon Bay, mentioned in the *Mabinogi of Math the son of Mathonwy*, which may be 'based upon a fragment of history that has survived from a remote past.'

We owe the legends of the sunken cities as they are today to seventeenth-century romantics and 'educated authors' of later centuries. Accounts of submerged lands in Holland and of the floods which occurred in 1607 in the estuary of the Severn may have contributed to the 'improvement' of the originally simple tale.

About the legends of Lyonesse and Brittany, Dr. North remarks shortly: 'That all these stories are related is evident, but whether because of independent derivation from a common source or because a story told in one region was carried to others is difficult to say.' Yet, a similar investigation of the southern coast of Brittany, where the inundations were of far greater consequence than those on the Welsh coast, may shed new light on the problem.

On all Celtic shores we must search for magico-religious attempts to arrest erosion or the rising of the sea level. To the instances given in *Folk-Lore* (Vol. LXIII) I would like to add the Celto-Roman temple near Pinnacle Rock on the north-west coast of Jersey, more threatened by erosion than any other part of the island. The study of such rites reveals whence T. L. Peacock's statement about 'thrusting the points of their spears into the earth to save themselves from being blown away' (p. 159) stems: he misinterpreted the Celtic custom of attacking with their weapons the waves, which they regarded as hostile.

Dr. North's discussion of the tidal range (p. 209) should lead to an investigation of the folklore connected with estuaries. The tidal range determined many landing places of early seafarers and there can be little doubt that they were magically protected by graves or sanctuaries. Besides, rivers with salt water were believed to be specially close to divine beings. The building of New Grange, e.g., may well be connected with the tidal range. Innumerable questions confronting the modern folklorist can only be answered by a geographer; but it will be difficult to find a more sympathetic collaborator than Dr. North.

E. ETTLINGER

**Atlas der schweizerischen Volkskunde, Part 2.** Fourth issue. Basel (Schweiz. Ges. für Volkskunde), 1957. Pp. v, 303-91, 14 maps.

147

In this latest issue Dr. W. Escher discusses the geographical distribution of traditional excursions in Switzerland. With the decrease in horses and the increase in motor cars, asphalt roads, and the more exciting forms of winter sport, such as skating and skiing, the formerly very popular excursions in sledges are diminishing. Miss E. Liebl accounts for the answers received on the questions: which are the most frequent Christian names and who chooses them (relatives, godparents or the clergy) as well as which stories are told to children about the origin of babies? Both schools of thought on the latter subject are mentioned: the older theory that these stories contain survivals of ancient beliefs and the claim by the late Professor von Sydow that from the educational point of view evasive (and often funny) answers had to be invented. Miss Liebl's investigation of the stories is divided into two groups: the first treats of the places from which the infants were fetched (Heaven and Paradise, stones and mountains, wells, ponds, plants, trees, castles, etc.). Of relatively recent date and mainly confined to the Ticino are the stories of infants which were bought (e.g. in shops or hospitals). The second group deals with the various infant-bringers: God, the angels, saints, midwives, etc. In the Wallis, where barren women go on pilgrimage, an actual hermit or hermits in general are referred to. Stories about the stork, which usually brings the infant down the chimney, have been found to be merely some 70 years old.

E. ETTLINGER

**Village in the Vaucluse.** By Laurence Wylie. Cambridge, Mass. (Harvard U.P.) (London, O.U.P.), 1957. Pp. xiii, 335. Price £2 5s

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Villagers in Provence are no less suspicious of strangers than those in rural England. Professor Wylie, who is Chairman of the Romance Language Department of Haverford College, explained his stay by telling them 'I am a teacher of French civilization. I have never lived in the South of France or in a village. We should like to live here a year to see what life is like.' His book tells us 'what life is like' in Peyrane (a pseudonym), an agricultural settlement of just over 700 people in a little-known region of south-eastern France. (Such studies are far too rare. In Britain at least, the vast majority of sociologists labour in the shadow of the gasworks, while the anthropologists find it necessary to travel to Central Africa to choose a village.)

As a portrait it is remarkably successful: Professor Wylie's skill



in describing the minutiae of village life must be the lasting envy of every fieldworker. It is a vivid, evocative account that holds the reader's attention like a vice. Acute observation and a keen eye for telling detail are evident on every page.

Professor Wylie has a gift for 'getting inside' a community that must be very rare in university departments of language, and uncommon enough in departments of social science. It is this gift which makes the study a notable achievement and at the same time a tantalizing challenge to the sociologist. A third of the book deals with childhood and adolescence, but there is virtually no treatment of family structure or kinship. The Peyranais are mostly farmers, yet there are only passing references to land-holding and inheritance. There are many brief passages about important aspects of village life which show great perception, but which fail to analyse adequately their social content and interrelationships. For example, there is an interesting account of *la veillée*, the informal meeting of friends in each others' houses, but it is an account which leaves many questions unanswered. Do these visits form a regular pattern? Are there distinct groups of 'friends' as defined by such visits? Are such groups large or small? Do groups overlap? etc., etc.

It is, however, easy enough to discover omissions in a general survey of this kind. Professor Wylie's book remains a most valuable study, which will be welcomed as an important addition to the literature on small communities in the Western world.

W. M. WILLIAMS

**Handbuch der Glockenkunde.** By Fr. Winfred Ellerhorst, edited by Fr. Gregor Klaus. Weingarten (Martinus-Buchhandlung), 1957. Pp. 248, 40 plates, 162 text figs.

149

Much information is here given on the acoustic and technical problems confronting bell-founders and good advice on the care of bells, and a few rather vague paragraphs are devoted to their historical and decorative development. The word *cloca* appears for the first time in A.D. 736 in a letter written by St. Boniface. The word is generally believed to derive from the Old Irish *clóc*, though Professor H. Schuchard pointed to the Romance *claccare* and the even more likely derivation from *cochlea*. The first European use of bells is attributed to the Romans and the Greek evidence, from the fifth century B.C. onwards, is missing. It is mentioned neither that bells of sheet iron were known in Ireland about the first century B.C. nor that early Irish bells were clapperless and struck from the outside. Charlemagne prohibited the baptism of bells in order to stop them being rung against hailstorms. Rather surprising is the list on p. 188 which registers Aberdeen, Dublin and Dumbarton under the heading of 'England.'

These shortcomings may be due to the untimely death of the author and the editor's inability to trace the references. The bibliography is fairly substantial, though, e.g., *Les Annales de Didron*, E. Coussemaker, H. Hickman, A. S. Pease, and H. Simbriger, are missing.

In spite of this well produced book, a new revised and amplified edition of H. Otte's erudite works on *Glockenkunde* is still badly needed. It will require far more extensive studies and a completely unbiased outlook.

E. ETTLINGER

**Sicily Before the Greeks.** By L. Bernabò Brea. London (Thames & Hudson), 1957. Pp. 258. Price £1 15.

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The 'Ancient Peoples and Places' series sets out with the double aim of providing in each volume 'an up-to-date archaeological textbook and a clear, readable account for the interested layman.' It is not an easy aim to achieve, and in this book Professor Brea has catered more for the archaeologist than for the layman. The former, too, will be less worried than the latter by occasional rather stilted turns of phrase in the translation. But since pre-Greek Sicily has little to attract popular attention compared with the subjects of other works in this series, I feel that the mistake is on the right side. Whatever the layman may think of it, the archaeologist can only offer whole-hearted praise.

This concise, authoritative and well illustrated account fills a badly felt need. The sequence of cultures in Sicily was summarized by the author in the *Sixth Annual Report of the University of London Institute of Archaeology* in 1950, but the fuller treatment here given

adds considerably to that, and includes the advances made in the last seven years, such as the discoveries in the Canicattini caves.

Even more urgently needed was information about the author's vitally important work on Lipari, hitherto available only in Italian in periodicals not easily obtainable in this country. These excavations, revealing an unbroken stratified sequence from Neolithic to Classical times—and only those who have tried to work on Italian problems will realize how serious the lack of stratigraphies in that country is—provide probably the greatest single advance in Italian prehistory yet made. It is encouraging that it should be an Italian who makes it, rather than a Peet or a Säflund from outside. Yet at one point he follows a red herring which has always troubled Italian prehistorians—the correlation of archaeological material with peoples recorded in the classical sources. If the 'Ausonian' Apennine material of Lipari comes from Apulia rather than Campania, and parallels are even closer higher up the Adriatic coast, surely we should cease calling it after a Campanian tribe, whatever Diodorus Siculus may say on the subject? However, this detracts little from the merit of a very useful book.

D. H. TRUMP

**The Rock Shelter of La Colombière.** By Hallam L. Movius Jr. and Sheldon Judson. *Amer. Sch. Prehistoric Res. Bull.* 19. Cambridge, Mass., 1956. Pp. x, 176, 52 illus.

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In 1915 Dr. Lucien Mayet, senior lecturer in Anthropology and Human Paleontology at Lyon University, and M. Jean Pissot published a report of their excavations at the Abri-sous-Roches Préhistorique de la Colombière, Près Poncia, Ain (*Ann. de l'Univ. de Lyon*, N.S., Vol. I, Part 39); a report of 193 pages with 102 figures and 25 photographic plates containing 684 figures. The colotype reproductions of stone tools in these plates can justly claim to be finer than any published today. Chapters were devoted to the geology and stratigraphy of La Colombière, the animal remains, the flint industry, and the prehistoric art found at this site. Mayet and Pissot's report has lately been supplemented by a new study of the site and its surroundings, published in the volume under review, also the result of a joint investigation by an archaeologist and a geologist, together with 186 extremely fine illustrations of hitherto unpublished flint and bone artifacts from the earlier excavation, the work of M. Humbert of the Musée de l'Homme, and a report of the animal remains found on the site (by Henri Gautier) in the light of modern knowledge. Considering the high standards of both the 1915 and 1936 reports, La Colombière must be among the best-published Upper Paleolithic sites in France.

The 1956 report is divided into five parts; a stratigraphical study, a geological survey of the surrounding territory, the fauna, the human artifacts and 'Summary and Conclusions.' The geologist is joint author of the section on stratigraphy, an example which deserves to be followed in more cases, and was overwhelmingly necessary at La Colombière, where the extant occupational deposits were contained within water-laid sediments associated with, and dated by, the 23-metre terrace of the River Ain. La Colombière had originally contained overlying Magdalenian and neolithic(?) deposits, but these had been removed from the site in the nineteenth century, after which the site was assumed to be of little importance. Mayet and Pissot found an earlier deposit containing relics of three transitory occupations of Upper Perigordian (then Upper Aurignacian) character, and hearths, associated with the splendid engraved pebbles to which they devote so much attention. These authors recognized the relationship between the sands within the rock shelter and the Pleistocene river terrace without, and proposed a chronology founded upon this fact. Movius and Judson found ample confirmation in the stratigraphy defined in the earlier report. They were also able to define the character of the deposits in greater detail. The geological investigation enabled the position of the 23-metre terrace of the Ain to be defined in local terms, but owing to our lack of knowledge of the oscillations of the last glaciation nearer the Alps a definition of the absolute date in North European geological terms could not be attempted.

The main archaeological part of the report is concerned with a detailed study of the flint work, nearly all found in the earlier excavations, associated with the earliest layers at the site, the greater part being believed to come from the layer with hearths.



Dr. Movijs finds that this belongs to a variety of the Upper Perigordian not exactly paralleled in Western France, and explicable in terms of local evolution. Finally, as a reward for their patient studies both on the site and among the unpublished records and

finds of the earlier excavations, a further magnificent engraved pebble turned up in the 1948 excavations at La Colombière, in an undoubted association with a hearth of the earliest Perigordian horizon.  
G. DE G. SIEVEKING

## OCEANIA

**Ancient Voyagers in the Pacific.** By Andrew Sharp. London (Penguin Books), 1957. Pp. 240, 12 plates, 3 maps. Price, 3s. 6d.

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The theme of this very interesting book, first published in 1956 as Memoir No. 32 of the Polynesian Society, is that there is little foundation for the belief that the peoples of Oceania, and especially the Polynesians, were great explorers who set out on long overseas voyages to discover new lands or who maintained contact with distant groups. While admitting that deliberate voyages by bands of exiles, caused by population pressure or political upheaval, did occur, the author suggests that the significant factor in the peopling of the Pacific islands was the frequency with which canoes were blown off course while on short inter-island crossings. He bases this belief on two main grounds: the inadequacy of navigational knowledge (relatively highly developed though it certainly was); and an examination of the earliest accounts by Europeans, which show a high proportion of accidental voyages and few authenticated deliberate ones over long distances.

In one of the most interesting chapters Mr. Sharp discusses the limitations imposed on navigation by the cultural level reached by the Oceanic peoples, showing the impossibility of fixing position after being blown off course or of maintaining a course if the stars are obscured. Set and drift—that is, deviation caused by current and wind—are unknown quantities. To set off from a continental coastline, knowing that it is impossible to miss land on return, is one thing; to set off from a small island group and hope to return to it is quite another. In several instances Pacific islands were discovered by Europeans in the days of sail and their positions fixed by instruments, but they could not be found by subsequent voyagers. The fact that Polynesians sometimes had knowledge of distant groups at the time of European discovery shows only that voyages had been made from those groups, not that the voyages were deliberate or that the travellers returned home. Thus, the Hawaiian traditions apparently referred to Tahiti, but the Tahitians had no knowledge of Hawaii. The voyages should not be regarded as drift voyages; the crews were usually in full control of their vessels and set what they considered to be the best course possible in the circumstances. As the author points out, the single-outrigger or double canoe sails best across the wind, and as the winds prevailing in central Polynesia are easterly or (according to season) westerly, such unintentional voyages would tend to reach the marginal groups to north and south. The general direction taken by most castaways was probably westward rather than eastward, so that they reached islands already inhabited; but westerlies are common enough to account for the spread of population eastward. To take an example from further west, there are in the British Museum the prow ornaments of a canoe from the Laughlan Is., off the east end of New Guinea, which reached Vella Lavella in the Solomons. Another factor is that the vessels were sufficiently well constructed to survive a long voyage: during the second world war a Gilbert Islander reached the Admiralties after seven months at sea, during which he lived on fish. The limiting factor, if the boat survives gales, is probably the durability of the lashings when they are continually wet. A few quotations summarize the conclusions of this chapter: 'The Polynesian and other Pacific Islands vessels, like all other early types of vessels, were unable to hold a course in a gale. . . . If they were caught by a storm, they not only lost their bearings by the stars and sun, but were liable to be swept away. . . . The reason, therefore, why the East Indian and Pacific peoples colonized distant islands was not because their vessels and navigation were good enough to take them to such islands, but because they were not good enough not to. . . . When the gale blew itself out they might be hundreds of miles to sea in unknown waters with no way of fixing their position, or

plotting their course, or knowing the position of their home island or their destination' (pp. 38f.).

The other evidence for and against the views summarized above comes principally from two fields: Polynesian tradition, and the distribution of items of material culture, especially of food plants and domestic animals. Mr. Sharp's conclusion about the traditional material is that the pre-European traditions were vague and poetical rather than historical, and that the more precise traditions which contain names of distant places and sailing directions were mostly collected after the middle of the nineteenth century and reflect knowledge deriving from European contact and sometimes elicited by leading questions. This seems to be the most doubtful part of Mr. Sharp's case. It is true that knowledge of the other groups gained from Europeans was eagerly assimilated and that the stories were living things which grew and changed. It is also true, as Lord Raglan has pointed out, that the Polynesian traditions should be compared with the Arthurian cycle and not regarded as unwritten history. They must be used for historical reconstruction, if at all, only with the greatest circumspection. But the genealogies would appear to be in a different category. No single one can stand by itself, but when in a group of them substantial agreement appears, and there is on evidence that they are derived from a common post-European source, then surely they cannot be dismissed. An interesting short paper by J. B. Robertson in the *Journal of the Polynesian Society* (Vol. LXV, No. 1, p. 45) describes the application of statistical methods to the analysis of a group of Maori genealogies, with positive results.

The evidence from the distribution of food plants and animals seems mostly to support the argument. The random distribution of such economically important animals as the pig and the chicken would not seem to suggest deliberate colonization. The pig was present on Rarotonga and Atiu of the Cook group, but not on Mangaia or Aitutaki; it was not known on Niue, which would surely have been a port of call for deliberate voyages. The deductions to be drawn from food-plant distribution are less clear. The author makes an important distinction between plants which could be propagated by means of edible parts—e.g. tubers or nuts—and those which were propagated only by offshoots or cuttings, such as the seedless breadfruit and plantain. The former clearly might be on board any fishing canoe which was blown away; the latter suggest at least intentional transfer, even if the eventual planting was not in the place intended. Perhaps Mr. Sharp might have recast this part of his argument if he had had a chance to read the late Professor E. D. Merrill's *The Botany of Cook's Voyages* (reviewed in MAN, 1956, 125).

*Ancient Voyagers in the Pacific* is the result of a thorough and original re-examination of the whole question. It is closely reasoned throughout, and one can mention only the salient points in a review. It is possible to disagree with Mr. Sharp on details and to believe that on occasion different conclusions could be drawn from his evidence. For example, if accidental voyages were so decisive why did not the Fijian culture influence that of western Polynesia more strongly? The Fijians were numerous and made exceptionally fine double canoes, and though they were by comparison unenterprising seamen this should have made little difference to the number of accidental voyages made by them. Second-class seamen in first-class boats would seem to provide the ideal circumstances for this kind of mishap. In fact the influence was mainly the other way, and the elements of Fijian culture that did reach western Polynesia were mostly carried by returning Tongans; there is no doubt—as Mr. Sharp says—that the Tongan voyages to Fiji were intentional. Perhaps the Lau group acted as a filter which caught up castaways from the larger islands. Most readers will make reservations, but



Mr. Sharp builds a strong case which seems likely to stand in its main essentials.

There are a number of illustrations which mostly add little to the text, useful maps, and a good bibliography and index. It must be a slip that makes the dodo a native of Madagascar.

B. A. L. CRANSTONE

**The Great Village: The Economic and Social Welfare of Hanuabada, an Urban Community in Papua.** By

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Cyril S. Belshaw, London (Routledge & Kegan Paul), 1957. Pp. xviii, 302. Price £1 10s.

Dr. Belshaw has presented us with an important and urgently needed volume on welfare economics, concerned with the adjustment of the Port Moresby Hanuabadans and their interaction with the wider European community. It is important because it gives us a detailed account of actual social conditions in relation to economic satisfactions, and is broad enough to have vital anthropological significance within an economic framework (e.g. pp. 4-9) which is not confined or rigid. It is urgently needed because too few anthropologists—at least in the Pacific region—have emphasized the economic aspect: and when they have, it has rarely, if ever, been oriented in the specific direction of welfare. Further, a detailed account of a village within the Western European orbit, as Hanuabada is, being subjected to constant intensive 'external' (and in a sense, internal) pressures at all levels, should be welcomed by both anthropologists and administrators. There is no parallel study of a relatively sophisticated community of New Guinea or Papuan people, as town dwellers. (Dr. I. Hogbin's Busama, in *Transformation Scene*, 1951, has a rather different setting; and as yet we have no anthropological studies of, e.g., Lae, Madang, Rabaul.) In his economic-anthropological analysis Dr. Belshaw, consistently with his view that an economic approach should consider not merely tangibles such as commodities and money but also intangibles, does not confine his discussion to production and ownership, income, budgets, the material level of living and trade. He is equally concerned with ceremonial affairs (the 'private' and 'public' ceremonies: marriage, house-warming, birth and death on one hand, and the dance, council celebrations, clubs, cricket on the other), marital relations, social living from birth to adolescence, religion, sorcery, law and order.

With so much of interest to hold the reader's attention, it seems almost ungracious to draw attention to a few minor points; but while conforming with this well established custom of book-reviewers, I should make it clear that they are indeed minor points, which do not detract from the overall value of Dr. Belshaw's contribution. On p. 1, e.g., we are told that the people of Hanuabada 'still lead a very primitive life'; from the content of the volume it is not clear what these 'primitive' elements are. If the measuring rod here is technological achievement or complex economic commitments this should be clearly stated; certainly in discussing a social situation of this kind the term becomes even more unsatisfactory than usual. We are told also (pp. 1f.) that Hanuabada 'is the only example we have in this area of a truly urban native community,' that 'problems of native urban growth are becoming increasingly important,' that 'urbanization in Hanuabada differs from that studied in other tropical towns.' In my view, we need more detailed examination of the empirical situation before we can use the term 'urban' in relation to Hanuabada. Certain criteria have been suggested by sociologists as a basis for distinguishing urban or city units from other types of settlement, and although these cannot be set out here I would say that only a few of them would apply.

Hanuabada, we read (p. 2), 'is still as much an organic unity as it ever was, and the traditional ways of behaving are still of basic importance beneath the overlay of Western culture that has in part been adopted.' On the other hand (p. 1) 'The people are wage-earners for the townsfolk [i.e., Europeans], and it is around this fact that their whole life is organized.' On p. 243 the loss of their 'traditional background' is made clear: Hanuabada has 'changed further than most other Papuan and Melanesian groups...'; 'further' means, one assumes from the context, more obviously in the direction of Europeanization. It seems to me, from reading this volume, that Hanuabada is far from being an entity having 'organic

unity,' but may rather be viewed as a section of the larger community consisting of Europeans, Hanuabadans and a motley of others—indigenes and immigrants—living within a system broadly oriented along lines of European emphasis. Certainly Hanuabada is part and parcel of the wider economic and political systems, and this is clearly the case too as regards a number of other aspects. By this I do not, of course, mean that 'unity' is synonymous with isolation, or with weakness of links with the outside world. It is obviously always a relative matter: and Dr. Belshaw is evidently thinking here mainly in terms of social relations, the focusing of attention on interpersonal affairs within the village (or cluster of villages) as against the restricted channels for interaction outside it. But even viewed as a 'minority' (in political terms) the Hanuabadans can still be said to be part of the whole. In this respect the definition of the term 'society' (p. 11) requires more treatment. Sociologically, I would like to have seen this problem given more prominence. This is the case, too, as regards change. Dr. Belshaw defends himself, in advance, by saying that this is 'not a study of the processes of culture change' (p. 2). Nevertheless, the problem of social and cultural change is quite inseparable from his study; it is part of the data with which he is dealing, and is a dominant thread running right through his book. In this context, it is important to know exactly what 'the traditional ways of behaving' are. From the evidence supplied it would seem that many of these are not really 'traditional' (if by this we mean something that is predominantly native or indigenous); they represent rather a reassembling of elements of both European and local derivation, involving something different from the European, perhaps, but not necessarily to be identified as traditional—except by nostalgically or otherwise labelling them with the stamp of 'genuinely native' as against 'European.'

But there is no time to dwell on points such as these. Dr. Belshaw has faced, as many anthropologists do not, the social responsibility and obligations of his discipline—that is to say, its practical application to problems of welfare; and in this respect alone, as Professor Firth suggests in his Foreword, the study is a significant one. Dr. Belshaw recognizes the dangers here (p. 3), and the absence of universal value categories of assessment; yet in such matters it is not always possible to avoid commitment in one direction or another, with all that this implies. 'Ethical relativity' may be 'rather stale,' as Firth puts it (p. vii), but it may still be valid in a modified form as a necessary counteraction to vague assumptions about 'human nature,' or attempts to set up absolute norms or standards. Dr. Belshaw, as I say, is well aware of this position. At the same time, some of us would not agree that 'In many respects, the comments of anthropologists on problems of welfare policy are analogous to the criticisms of art or music critics, who, without being artists themselves, have developed standards of appreciation which vary in objectivity according to the methods used' (p. 4). Of course, in this disclaimer and in the remarks leading up to it Dr. Belshaw obviously had his potential local readers in mind.

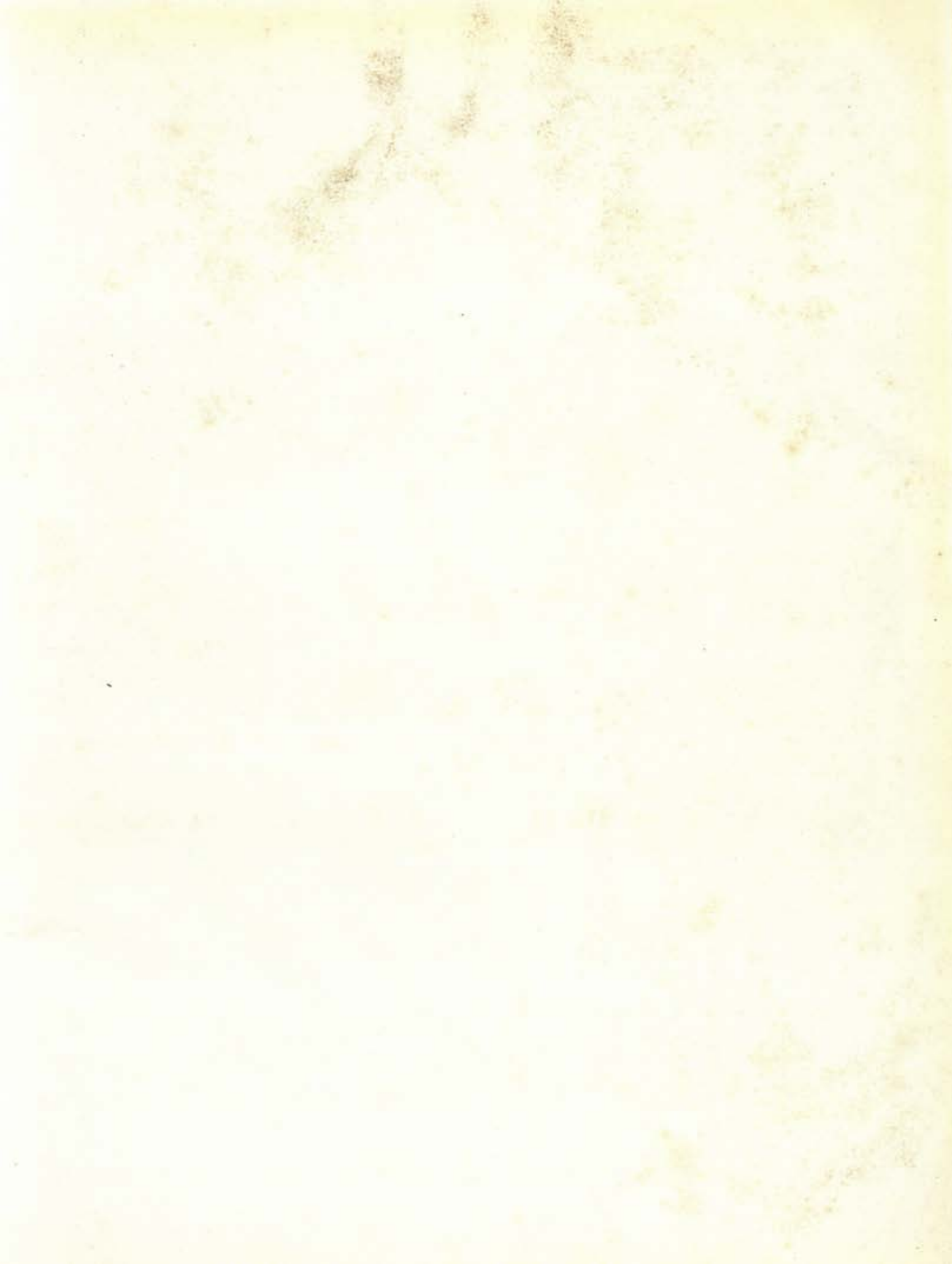
For anthropologists, administrators and students of social welfare, the text provides useful material all the way through. Relatively full tables of occupational distribution and wages received (etc.) are given (pp. 66-71), as well as patterns of expenditure (pp. 84f.).

The final chapter, *The Conditions of Welfare*, one hopes will be read particularly by Australian and New Guinea officials and administrators concerned directly with these problems. There is no polemic here to ruffle their sensitivity, simply a concise statement of local conditions, and the implications of these conditions, as they have emerged from the body of this study, showing what the trends are and what, given certain conditions, their outcome is likely to be.

Dr. Belshaw speaks of Hanuabada as having 'few fields in which there is unity': 'there is a lack of neatness in any picture which may be drawn' (p. 244). The points he raises here are directly relevant to Western-European-type society; for Hanuabada for all its external differences, its Papuan population, its kinship and its ceremonial interests and so on, is more 'European' than it is 'native.' But it is obviously not 'European' in the sense that the town of Port Moresby is; rather it is a combination of the two, the combination itself producing the difference so convincingly indicated in Dr. Belshaw's study.

RONALD M. BERNDT









# **A BENIN BRONZE PLAQUE REPRESENTING A GIRL**

Height 18 inches, width seven inches. Photographs: top left and right, Dr. Wolf Strache; bottom left, by courtesy of the Trustees of the British Museum



# ON A BENIN BRONZE PLAQUE REPRESENTING A GIRL\*

by

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**154** In the corpus of Benin antiquities brought to our knowledge by the British expedition of 1897, by far the largest typological group is formed by the great series of rectangular bronze plaques,<sup>1</sup> in number probably about a thousand. These works, which show a remarkable consistency of style and quality (even if few of them can stand very high as works of art), may be regarded as forming the canon of the art of the middle period at Benin, by reference to which many of the round sculptures can also be attributed to this period. From the traditional evidence and the probabilities of the stylistic succession, we may suppose that the middle period corresponded roughly to the seventeenth and the first half of the eighteenth centuries, and it seems likely that most or all of the plaques were made during the earlier part of this period, since they appear to have been dismantled from the mud walls of the Oba's palace about the middle of the eighteenth century and stacked away in a part of the compound where they were found by the British a century and a half later. We do not know how they were originally arranged on the walls of the palace, or why they were eventually taken down, though this may have been on the occasion of a rebuilding of the palace. The plaques appear to commemorate persons and events, though actual movement is but rarely represented. As many as seven or eight figures may be represented on a single plaque, though one, two or three figures are more common; animals, plants and artifacts are also represented. Most plaques fall into one of two sizes: the larger is about 17 to 19 inches high by 14 wide, the smaller about 16 by seven.

A striking feature of the rectangular plaques is that (with the exception now to be noted) women are never represented on them, though they are not uncommon among the round sculptures (chiefly in heads and figure groups for the queen-mother cult) and on the small shield-shaped pendent plaques. This is presumably significant of the lack of influence of women at the king's court as opposed to that of the queen-mother, which from the time of the early sixteenth century Oba Esigie onwards was held at Uselu, some four miles outside the city.

The plaque illustrated in Plate I is the property of Mr. A. Schwarz, a collector who has formed in the past few years an excellent collection of Benin antiquities. I understand that it had passed through the hands of more than one dealer since leaving Switzerland not long ago, but no information about its earlier history is available. When Mr. Schwarz brought it to the British Museum to

\* With Plate I

show me, my first instinct was to question its authenticity because the figure purported to be female. A number of plaques<sup>2</sup> exist which represent young naked boys differing in no formal respect from this figure except for the presence of the male genitalia, and it would be quite easy to remove these with a chisel and to stipple and colour the resulting flat surface so that the alteration would not be discernible from a superficial examination. However, the plaque was examined in the Research Laboratory of the British Museum, where it was established that the pubic area had not been tampered with in modern times and that the whole casting, as well as its patina, was a continuous one (there had seemed a faint possibility that a new middle section might have been 'burnt in' to the plaque).<sup>3</sup> That the casting as a whole is a genuine one from the same period as the other plaques I am entirely convinced.

The girl, presumably aged about 10 years, is shown carrying on her left shoulder what looks like a very young leopard but may equally well, or perhaps more probably, be an aquamanile in the form of a leopard, such as those illustrated in MAN, 1953, 261, fig. 3. Two naked boys similarly carrying leopards are represented on a plaque illustrated in José Pijoan, *Summa Artis: Historia General del Arte*, Madrid, 1948, Vol. I, fig. 294, and there attributed to the collection of M. Charles Ratton of Paris.

The all-over decoration on the girl's body presumably represents painting, such as is still practised by some Ibo girls on festal occasions, rather than scarification.

It may finally be noted that this plaque is exceptional also in the degree of vivacity imparted by the artist to the young girl's features, especially by contrast with the stolidity of the aforesaid representations of young boys.

## Notes

<sup>1</sup> For examples of plaques, and of Benin art in general, see especially Read and Dalton, *Antiquities from the City of Benin* . . . , British Museum, 1899; Pitt-Rivers, *Antique Works of Art from Benin*, London, privately printed, 1900; von Luschan, *Altertümer von Benin*, 3 vols., Berlin, 1919.

<sup>2</sup> For example, Read and Dalton, *op. cit.*, Plate XXV, 5; von Luschan, *op. cit.*, Plate XLII.

<sup>3</sup> By coincidence, shortly after first seeing the present plaque I was shown another which had been tampered with in precisely this way, although my inquiries satisfied me that this had been done, probably by the original collector soon after the expedition, not from any intent to deceive, but from a misguided desire to render it more suitable for display in the home. The chiselling showed up conspicuously under the ultra-violet lamp in the Laboratory, and the bronze paint was readily removed with acetone. The piece was sold at Sothebys on 2 December, 1957. Another emasculated plaque is in the Royal Ontario Museum of Archaeology at Toronto.



# SOME RECENT ARCHÆOLOGICAL WORK ON THE TANGANYIKA COAST\*

by

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Tanga, Tanganyika

**155** In 1953 the Conference on African History and Archaeology at the London School of Oriental and African Studies drew attention to the urgent need for a systematic survey of ancient sites in Africa. As a small contribution to this wide field, in the past four years I have devoted my leisure to the Tanganyika coast and adjacent islands. This is but a section of the East African coast, and of a cultural area which, broadly speaking, we may call the Western Indian Ocean.

In this I have not been alone. In the past ten years a number of articles on the history of the area have come from Sir John Gray.<sup>1</sup> J. S. Kirkman has excavated at Gedi and elsewhere on the Kenya coast with results which need no praise of mine.<sup>2</sup> But an inspiration to every worker in this field has been Gervase Mathew, who first visited the area in 1946 and realized its potentialities. We in East Africa owe him a great debt of gratitude. It was with the deepest pleasure that, on behalf of the Tanganyika Archaeological Society, in 1955, I welcomed him and Sir Mortimer Wheeler on a visit to Kilwa Kisiwani, the Mafia Islands and a number of lesser sites. Some of the results may be found in the Summer Number of *Oriental Art*, 1956, and in *MAN*, 1956, 61 ('The Culture of the East African Coast in the Seventeenth and Eighteenth Centuries in the Light of Recent Archaeological Discoveries').<sup>3</sup> Of this period it is unnecessary for me to speak, but I wish to direct attention to the concluding words of his paper<sup>4</sup>:

There are three immediate needs if we are ever to gain a clearer knowledge of the coast in the seventeenth and eighteenth centuries. The first is for the publication of a volume of selected documents and inscriptions. The second is for the full excavation of a manageably small site, perhaps the Palace quarter of Pate, though this will be merely destructive unless a full report is published. The third is for anthropological field-work in a Swahili coastal community, perhaps Pangani, or Mambrui or Witu. All three needs interlock. Africa can only be deciphered when history and archaeology are at least co-ordinated.

With these views I heartily agree and have work on the first desideratum already in hand. What I have to say now is largely as a gloss; and at the outset I wish to insist with great emphasis that without the fullest collaboration between historian, archaeologist, anthropologist and linguist, progress in all their fields in East Africa will be hamstrung. I shall speak first of the documents available, then of the coastal sites and their economic function, and then of new and important finds of coins, as well as of beads, pottery and porcelain.

We know nothing of the prehistory of the Tanganyika coast. No prehistoric site—using prehistoric in its ordinary

sense—has ever been identified. All the sites described by Sonia Cole are inland.<sup>5</sup> The first-century *Periplus of the Erythraean Sea*<sup>6</sup> has some brief but precious indications of trade relations between Arab settlers and native inhabitants; but their metropolis of Rhapta still defies discovery. The mediæval Arab writers speak of East Africa as Zanj, the Azania of the *Periplus* and the Zingion of Ptolemy<sup>7</sup> and of Cosmas Indicopleustes<sup>8</sup>; they have been summarized by Devic<sup>9</sup> and by Ferrand,<sup>10</sup> but do not provide even the semblance of consecutive history. In this sense the mass of East African coastal sites are prehistoric. Before 1500 only two sites have a recorded history. The first is Kilwa Kisiwani, the capital of a dynasty of Sultans founded c. A.D. 957,<sup>11</sup> who ruled the coast between the Mafia islands and Sofala; the other is Pate, off the Kenya coast, founded in 1204, which, in the later fourteenth century, attempted, with little permanent effect, to carve an empire for itself.<sup>12</sup>

After 1498, with the coming of the Portuguese, the quantity of documents becomes vast. Some have been published; but at the moment no less than 15,000 documents from the libraries of Goa, Lisbon and the Vatican are awaiting publication under the auspices of Central African Archives and the Arquivo Historico de Moçambique. The first volume was promised for 1956; that it has been delayed is a matter for regret. Nevertheless, such Portuguese documents as are available are more taken up with Portuguese anxieties than African affairs: it remains to be seen what new knowledge will be unfolded.<sup>13</sup>

Mombasa and Lamu have histories written by Arabs; they are largely concerned with the seventeenth to nineteenth centuries. Zanzibar has almost no historical documentation before the nineteenth century save by casual visitors who say more about themselves than the island. In Pemba two documents relating to mediæval times were found in 1956; I have not yet seen them and cannot estimate their value.

Possibly, even probably, there are documents still awaiting discovery. In 1931 Ingrams mentions a number known to him: but in spite of various enquiries none of these documents have been traced.<sup>14</sup> In August, 1955, I obtained a history of Kua, in the Mafia Islands, which survives as an oral tradition in a form which makes it apparent that it was originally a written text. In 1954 I verified the existence of a document in the possession of the chief of Mkwaja, 32 miles south of Pangani, which was first noted by Baker in *Tanganyika Notes and Records* in 1941.<sup>15</sup> Its story gives some idea of the difficulties facing us. I discussed the document with the chief and he promised me a copy. He claimed that it traced his family from the Prophet and their history as rulers in this area since c. A.D. 1300. Despite his promise, I failed even to gain the sight of it, nor was

\* With a map. A paper read before the Oxford University Anthropological Society, 8 February, 1957



a copy ever made. In fact, his were Fabian tactics, his promise a courteous refusal, an experience evidently suffered earlier by Baker, who wrote: 'The last Diwan of Mkwaja told me there was a fear that if the contents were made public outsiders might claim kinship with the Jumbe and intrigue harder to obtain the throne. Probably, too, they were kept secret that the custodian might evade awkward questions on the subject of the relationship or legitimacy of an individual.'<sup>16</sup> I need hardly underline that in this both Baker and I strayed from the path of the historian into difficulties that an experienced anthropologist might have evaded, and, given time, possibly overcome.

For the whole of Tanganyika much valuable material has been collected by District Commissioners in their District Books, relating mostly to the last century and a half; very little has been published. This traditional

material deserves to be seriously followed up, for it is reliable in that the collectors were concerned to establish accurately the claims of chiefs; the information comes from what we may call court circles.

While I would thus be the last to deny the possibility of finding further documents and oral traditions, I must enter a caveat against a number of oral traditions reported by R. Reusch in his *History of East Africa* (Stuttgart, 1954). They relate to mediæval Kilwa, and, he claims, expand and supplement the Arabic and Portuguese versions of its history. Sir John Gray and Dr. Roland Oliver have both criticized his work in detail.<sup>17</sup> Here I wish to make the point that Reusch gives neither the names of his informants nor how they gained their information. This is wholly unlike the method of the District Books. Reusch's punctuation does not indicate where the tradition he is quoting stops and his own comment begins. In short, his canvas is indistinguishable from his embroidery. And it is most curious that, although he claims to have collected new oral traditions in both Pangani and Kilwa districts, their very carefully compiled District Books have no record of these traditions.

It is not only in the strictly historical sphere that there is fresh material to be sought. Both Steere and Hichens have published poetry of the seventeenth to nineteenth centuries, of which historically the most important is the lament for Pate, *al-Inkishafi*.<sup>18</sup> Recently Dr. W. Whiteley, Secretary of the East African Swahili Committee, has made electrical recordings of poems at Tongoni, near Tanga; these take some eight hours to play, and it is evident that, when his transcriptions are complete, Swahili literature will have been considerably enlarged, if not enriched.

I think that I have said enough about documents and oral traditions to indicate how much remains to be done. There is a lifetime of work for many men. This is even more true of the sites. For the whole coast of Tanganyika, nearly 600 miles, only ten sites are recorded in print. Certainly these are the most important: they include the ancient capital of Kilwa Kisiwani, the palace town of Songo Mnara, the city of Kua on Juani Island, Kisimani Mafia—of which more anon—Tongoni, Kaole and Mwasani.<sup>19</sup> In the last four years my wife and I have carried out a systematic search which has added a further 52. If the map still shows few in the Southern Province and in the Rufiji delta, it is probable that the erosion of the coast in the first area and the constant changes of course of the river in the other are largely responsible. The survey has been essentially a surface one: it seemed more important to identify sites over a large area than to concentrate and dig at one or two localities. There is much to be learned from the surface, even if at present it is impossible to say when occupation began or to trace the history of a given site. It would be wrong to assert that the only mediæval sites are those where we have found Sung celadon or the earlier Ming blue-and-white. Some sites are littered with seventeenth-century porcelain; we do not know what lies underneath. In *The Times* of 24 September, 1955, Sir Mortimer Wheeler remarked that the history of East Africa is written in Chinese porcelain. It can be found on



FIG. 1. EAST AFRICAN COASTAL SITES



the coast wherever there has been human occupation, from the Sung dynasty to the nineteenth century, when it was elbowed out by poorer European wares.

At first sight, then, it would seem comparatively easy, by a small amount of excavation, to establish the chronology of a coastal site. But reference to the detailed description of pottery and porcelain finds in Kirkman's *Gedi* shows that at present it is not possible to date more than a very few: with a single exception, Kirkman dates none of his finds within a narrower margin than 100 years.<sup>20</sup> The Chinese porcelain, and the glazed pottery which came from Syria, Mesopotamia and Persia, are almost all common wares which cannot yet be dated. The porcelain is not the magnificent ware made for the Imperial family or the wealthier classes. It is export ware, cheap, though not nasty. It is not what you can expect to find, and date, in a museum collection in Europe.

All the 62 sites on the Tanganyika coast are linked by a common use of imported porcelain and pottery, as well as other characteristics. All imported the same kinds of beads, of which there are important collections in the museums of Dar es Salaam and Zanzibar. There are common architectural traits, although at present it is not possible to date them or arrange them chronologically. The large centres, Kilwa Kisiwani, Songo Mnara and Kua, have palaces. Inevitably, there are Friday mosques. At these three sites a number of stone houses survive; Duarte Barbosa<sup>21</sup> says that there, at Mombasa and Malindi they were of several storeys, but at the last two there are no survivors. There is no evidence for buildings of more than one storey at Gedi, not even the palace. At all the remaining sites in Tanganyika I have been unable to find the ruins of any stone houses, but at Mbweni and Tanga there can be seen traces of hutted buildings associated with porcelain of the sixteenth and seventeenth centuries. Probably—and this is also Kirkman's view as to Kenya—outside the important centres the only stone buildings were the mosque and the tombs of the wealthy. The villagers lived in huts, but occasionally a wealthy man, like the chief of Mkwaja, might have a stone house; this was destined to be enlarged and remodelled, c. 1890, as a German fort.

What was the *raison d'être* of all these settlements? And on what information can a systematic chronology be based? The answers can be partly provided from known documents, buttressed, in the case of the second question, by new and important finds of coins.

The *History of Kilwa Kisiwani* exists in an Arabic abstract made in 1862, and, in another abbreviated form, in Barros's *Asia*, published in 1552. Both depend on a much larger work, now lost, the *Summa al-Kilawia*, *The Tradition of Kilwa*. They trace the foundation of the state c. 957 as a trading centre by Perso-Arabs from the Gulf. The foundation was successful enough to expand and conquer Mafia. For two centuries it had no great prominence, but at some time early in the twelfth century the ruler established a monopoly in the gold trade with Sofala, which hitherto had been conducted by Mogadishu. The details are not as clear as one would wish, but Kilwa was greatly enriched, town walls and a palace fortress built;

the Sultan was 'master of the trade of the coast' from Pemba to Sofala.

In all this area Sofala and the region close by was the only source of gold: it came, so far as can be ascertained, from alluvial deposits in its hinterland of present-day Moçambique and Rhodesia. It is clear that trade in the rest of the area was in other commodities, desirable enough in the fourteenth century for Pate to attempt to carve herself an empire along the coast, northwards to Mogadishu, and southwards, it was claimed, up to, but not including, Kilwa. On this war and its consequences the Pate history is singularly uninformative. The Kilwa history is silent, but it is certain that in the fifteenth century Kilwa was in decline: it was torn with domestic strife, while new trading cities, Pate, Malindi, Mombasa, were rising to power and wealth.

What was the basis of their new wealth? It was not gold: Kilwa held Sofala until the Portuguese seized it in 1506. From the *Periplus* onwards, of all the Arab writers, none fails to mention ivory, which Masudi says was exported to India and China.<sup>22</sup> High officials could attend the imperial court only in ivory palanquins—the use of an iron palanquin was undignified. Ibn Battuta mentions ivory as an important commodity at Kilwa.<sup>23</sup> Soares, the ablest of the Portuguese factors at Sofala, averaged an annual collection of 51,000 pounds of ivory over five years,<sup>24</sup> an immense quantity representing a yearly slaughter of 1,100 or 1,200 elephants.<sup>25</sup> Even if elephant were more plentiful in mediæval times than now, to collect so much would imply hunting over a very wide area.

Then there was the slave trade. We should not look at it through the eyes of Livingstone, or Kirk, or the late-nineteenth-century missionaries. Evidently there had been a very numerous export of slaves from the Zanj countries prior to the ninth century: this alone would account for the revolt of the Zanj slaves which threatened the Caliphate in southern Iraq from 869 to 883.<sup>26</sup> But in the middle ages the Arabs and later the Portuguese writers rarely mention it, many of them not at all. It may be suggested that the Zanj revolt depreciated the slaves as a commodity, so that in the middle ages the slave trade in East Africa was only a small affair, minor as compared with the ivory trade; it came again to the fore only towards the end of the seventeenth century after the Omani had taken Mombasa.<sup>27</sup>

The ivory trade, then, would explain how the numerous small settlements met their import payments, Kilwa and the other cities, including those in Mafia, acting as *entrepôts*. The small settlements provided contact with the immediate interior. None of them were defended by walls—at least not before the Zimba raids of the late sixteenth century<sup>28</sup>—and their existence could hardly have continued had they not lived peaceably with the neighbouring tribes, a further indication that the slave trade was not extensive.

A Chinese writer who died in 863, before the Zanj revolt began, draws a remarkable picture<sup>29</sup>; how he obtained his information is unknown.

The country of Po-pa-li is in the south-western sea. The people do not eat any of the five grains but only meat. They often stick a needle into the veins of cattle and draw blood



which they drink raw, mixed with milk. They wear no clothes except that they cover the parts below their loins with sheepskins. The women are clean and of proper behaviour. The inhabitants themselves kidnap them, and, if they sell them to foreign merchants, they fetch several times their price. The country produces only ivory and ambergris. If Persian merchants wish to go into the country, they collect around them several thousand men and present them with strips of cloth. All, whether old or young, draw blood and swear an oath, and then only do they trade their products. From olden times on they were not subject to any foreign country. In fighting they use elephants' tusks and ribs and the horns of wild buffaloes as lances and they wear cuirasses and bows and arrows. They have 20 myriads of foot soldiers. The Arabs make frequent raids upon them.

Both Somalia and Kenya have been suggested for this country<sup>30</sup>: for myself, I would suggest Tanganyika, in that there is no great waterless zone behind the coast, making life impossible for pastoralists. But what is remarkable about this account is that it is so like the present-day Masai, at least in most particulars<sup>31</sup>: it is impossible to say what their habitat was at this period.

The Kilwa *History* speaks of raids into the interior, which it and Ibn Battuta call *Jihad*—holy war. No doubt such raids maintained the slave supply; more important, I suggest, was the need to keep access to ivory open and to expand the area of collection. How wide was this area?

It is difficult to accept Coupland's view that it was only after 1825 that a route from Zanzibar to the Great Lakes was opened.<sup>32</sup> It may be true from the point of view of the emergence of Zanzibar as a principal emporium: for the earlier period, it is contradicted by al-Idrisi's knowledge, in the twelfth century, of the existence of the Great Lakes.<sup>33</sup> Burton and Stanley give details of their halting places: no one has searched these routes for material evidences of their use. The same is true of Burton's route from Pangani to Vuga, the capital of the Sultan Kimwere which he visited 100 years ago in February, 1857. Burton's great journey started from Bagamoyo; alternative coastal termini for the route to Lake Tanganyika were Kunduchi and Mboamaji. At Kunduchi I have found some remarkable inscriptions of the Sultans of the eighteenth and nineteenth centuries, whose family name, al-Barawy, suggests their origin from Barawa, in Somalia. At Mboamaji the mosque has an inscription dated 1607. Moreover, further north, at Tongoni, Ndumi and Ras Kikogwe are mosques and tombs all of which are certainly earlier than 1500. Surely Coupland cannot be correct.

Moving southward to Kilwa, there is yet clearer evidence. In 1882 Lieutenant J. B. (later Admiral Sir John) Eustace visited Kilwa and was told of a chain of forts of great antiquity which stretched 20 marches inland.<sup>34</sup> We must forgive a sailor for not giving a land direction: not one such site is known. Apart from the well-known nineteenth-century slave route, there are earlier indications. In 1616 Gaspar Bocarro, a Portuguese, marched from Tete to Kilwa over what seems to have been a well established trade route.<sup>35</sup> In 1331 Ibn Battuta had a conversation with a merchant who told him of known lands a month-and-a-half's march beyond Kilwa, a plain indication of a land route. Later, in 1352, when he visited Gaogao in West

Africa, he found Maldivian cowries used as currency, the same variety as that found at Gedi.<sup>36</sup> We do not know what route was taken or how ancient the trade was; assuredly it passed overland and not through the Mediterranean. A last pointer, not necessarily implying a connecting route, but perhaps rather a common source, is to be found in beads: in 1947 Miss M. F. E. Pelham-Johnson, O.B.E., found more than 10,000 beads at Kilwa, similar to those already in the museums of Dar es Salaam and Zanzibar. The frontispiece of Miss G. Caton-Thompson's *Zimbabwe Culture* shows an 80 per cent. correspondence.<sup>37</sup>

If the main tribal dispositions are known from the seventeenth or eighteenth centuries, there is little knowledge of those of earlier times. It would seem scarcely credible if patient work in the future did not trace the inland connexions of the coast, unfolding, despite the comparative silence of the documents, the history not only of the coastal hinterland but far beyond, by a study of material remains. The crucial problem will be that of chronology, for even if bead finds can already be found to overlap, and if in the future the same may be expected from pottery and porcelain, none of this material can at present be dated satisfactorily. To these problems a key lies ready in the shape of coins, and here recent discoveries have been numerous. So recent are they that less than half the coins found in East Africa have been published. They are of such importance that I must treat them in some detail. They suggest that from the second century B.C. until the end of the fifteenth century it will be possible to date the strata of certain coastal sites within very narrow margins. If in this way we can date pottery and porcelain, it should be possible to correlate the sites of the interior.

Coins were first reported at Kilwa in 1909, but not until 1936 was it shown, by Dr. J. Walker of the British Museum, that the Sultans of Kilwa issued coinage.<sup>38</sup> We have at present the coins of seven Sultans whose reigns can be dated with near accuracy from the *History of Kilwa*.<sup>39</sup> There are also coins of other mediæval East African rulers which have been found at Kisimani Mafia and Zanzibar: their names occur in no known dynastic lists, but are similar in type to those of Kilwa.

Since the spring of 1955 I have been privileged to be the first to examine the collection of the Beit al-Amani Museum, Zanzibar, and, more recently to see part of the collection of the Museo della Garesa, Mogadishu, which had been sent to London. I have also examined a number of private collections. The result has been the discovery of Roman, Parthian, Sassanian, Byzantine, Umayyad, Abbasid and Mamluk coins, as well as the first Chinese hoard to be found in East Africa, together with much new information on the Kilwa coins.

To discuss the new situation I must go back a little. In 1912 a Captain C. E. Hayward visited Bir Gao, or Port Durnford, in Jubaland, now Italian Somalia. He made his servants dig over the site, and obtained 85 coins, which were published in 1932.<sup>40</sup> There were 17 Ptolemaic pieces and five uncertain of the first to third centuries B.C.; six represented the Roman Emperors from Nero to Antoninus Pius; the close of the third and the opening of the fourth



century was the richest, with 46 pieces ranging from Maximin II to Constans. Finally there were six Mamluk pieces and seven of Egypt under the Turks. In *The Times* of 24 September, 1955, Sir Mortimer Wheeler described his adventurous journey with Gervase Mathew to revisit this site: it is a pity that they had not Hayward's luck.

In addition to the Port Durnford Hoard, a single Ptolemaic coin has been reported from near Dar es Salaam, while Charlesworth has listed a number of stray Roman coins in Africa, including both Nairobi and Cape Town.<sup>41</sup> In his *To the Mysterious Lorian Swamp* Hayward states that he also found a vessel like an ancient Greek amphora at Port Durnford<sup>42</sup>; it got broken on his return voyage, and he actually threw the pieces away. Whether it was Greek or not, my personal conviction that we shall yet find the Africa of Græco-Roman times was much fortified by finding in the Beit al-Amani Museum three small packages of coins, all with the typical local dirt adhering to them; unfortunately their accession had not been properly catalogued. The first envelope was marked 'Ctesiphon.' It contained three Parthian issues, one of the first century, one of the second, and one uncertain. There were two Sassanian pieces, one of Ardashir I (A.D. 212-41), and one uncertain. The other two packages appear to come from Ndagoni, Pemba Island, a site at which much ware of the Sung dynasty has been found.<sup>43</sup> I say 'appear' because some pottery sherds in the same packages are marked 'Ndagoni': the museum records are silent. In these envelopes, uncleaned and unidentified, were 17 coins. The earliest were an uncertain Syrian or Palestinian piece of the second century B.C. and another uncertain Hellenistic. There was a long gap to Diocletian and Licinius II, and again to Justin I and Justinian I, all represented by single pieces. There were two Umayyad pieces c. 700, one Saljuq, four Mamluk, and one uncertain Mongol of Persia, with three uncertain pieces.

I do not wish to claim too much for these coins. A single woodcock does not make a winter. But taken with the Port Durnford hoard, and the fact that several Mamluk and Mongol coins have appeared among hoards of Kilwa coins, we may expect such pieces in future excavation. If these lie on the surface, what is underneath?

Then there are Chinese coins. They have been found in twos and threes all along the coast from Mogadishu to Kilwa: in view of the heavy imports of porcelain, it is not surprising. They should not lead us to suppose direct contact: there are only two recorded Chinese voyages to Africa, both in the fifteenth century. Since the Ming records treat them as extraordinary feats of navigation, they must be regarded as rare events.<sup>44</sup>

A very remarkable find, still unpublished, was made at Kajengwa in Zanzibar in 1946. An African, digging his plantation, turned up, he estimated, 250 coins. His friends and neighbours ran to share his fortune, but the intervention of the District Commissioner enabled 176 pieces to be recovered. There were four pieces of Kao Tsung (618-627), the earliest Chinese pieces yet found in East Africa: the rest of the hoard represented every ruler of the North and South Sung dynasties from 998 to 1275.

It is true that in the eighteen-nineties Ming and Sung coins were still current in the bazaars in China.<sup>45</sup> But this hoard stops short at 1275, a strong reason for supposing it to have been deposited in the thirteenth century. There are no ruins at Kajengwa to account for it. There are many possible lines of speculation, but I think that we can at least expect further finds. Taking into account all the Chinese finds up to the present, every Chinese ruler from the seventh century is represented in East Africa; in excavated strata they can provide valuable *termini ante quos non*.

The Beit al-Amani collection also produced evidence of a local Zanzibar coinage in the fifteenth century. The Uroa hoard, of 3,204 pieces, and a number of smaller hoards showed that in the fifteenth century three rulers unrecorded in dynastic lists made local issues: Ahmad ibn al-Husain, Ishaq ibn Hasan (both of whose coins Walker first recorded) and al-Hasan ibn 'Ali, whose coins were first found in the Uroa hoard. While they can be dated from the presence of a small proportion of fifteenth-century Kilwa coins, they are not found outside the island. It is known from the *Kilwa History* that fifteenth-century Zanzibar was gaining in importance, for two attempts are mentioned to foist would-be usurpers on Kilwa.

It does seem strange that at present we have the coins of only seven Kilwa sultans. Al-Hasan ibn Talut (1277-94) and Sulaiman ibn al-Hasan, his son (1294-1308), issued coins; the latter had two sons who succeeded him, but we have coins only of the younger, Daud ibn Sulaiman (1308-10; 1333-56). Then there is a gap until Sulaiman ibn al-Husain (1364-66) and another until Muhammad ibn Sulaiman (1412-21). At the end of the century there were issues by 'Ali ibn al-Hasan (1478-9) and al-Hasan ibn Sulaiman (1479-85; 1486-90). I shall try to account for this, for it is of no small importance.

In 1955 the Liwali, or principal native magistrate, of Dar es Salaam told me that in 1939 he was shown some coins from Kilwa bearing the names Ismail and Ibrahim; he did not recollect the patronymics. Only one Ismail ruled Kilwa, Ismail ibn al-Husain (1442-54); there were two Ibrahims, one from 1490 to 1495, while another usurped the throne c. 1500. So there is a high possibility that excavation may add to or even complete the series; and this view is fortified by the fact that so far every traceable find has been a surface find made under somewhat peculiar conditions. I have only been able to trace a small number of the finds because the Zanzibar and Dar es Salaam museum records are deficient.

In 1955 I spent some time at Kilwa enquiring where the people had picked up coins. The vast majority came immediately from the west of where, in 1502, Francisco d'Almeida razed a number of houses to build a fort which is still standing. In the course of four-and-a-half centuries the flattened area has been eroded by the sea; it resembles a low cliff, but is composed of soil and sand drift. Buildings, or rather their remains, show in its side.

The great Mafia hoard of 3,661 coins published by Walker in 1936 came from the south-west point of the main island, known as Kisimani Mafia. I have traced a further



567 pieces from the same site. Now Walker only says that they were collected there by German troops; I have no doubt that they found them as my wife, Gervase Mathew and I have done. It is truly an amazing site: only two buildings are visible, one like a fortification on the shore, the other part of a house embedded in the cliff; close examination shows that it is no cliff in the ordinary sense, but is made up of successive layers of occupation, clearly demarcated by pieces of pottery and porcelain, occasional coins, cowries and fish shells. The numbers of distinguishable strata vary from three to five: at the base can be seen the same pottery as that of the lowest stratum of Gedi; at the top is Chinese porcelain of the eighteenth and nineteenth centuries. It is now clear from the Eustace diary that much of the town was washed away by a cyclone in 1872. It is mentioned in the *Kilwa History* as in existence c. 990. The tragedy is that twice a day the side is washed by the tides—each day bringing down pottery, porcelain and coins. Apart from a unique opportunity to link them together, its progressive destruction makes it an urgent case for excavation.

In spite of the great hoard which I mentioned at Uroa, there are no visible remains there, nor at Kajengwa; no other Zanzibar finds can be traced to sites. At Kua, on Juani Island, Mafia, there have been a few small finds: here are the ruins of a large town still standing, with a palace, seven mosques and some 30 houses. Pemba has the promising site of Ndagoni. The fortuitous manner in which almost all the coins have so far been found in eroding cliffs is, I think, the reason for the failure to find a complete Kilwa series: I believe that if Kisimani Mafia, Kua and Kilwa itself were excavated, this might be done.

Since Gervase Mathew specifically suggested that Pate be excavated, I would make a few comments. Like Kilwa, Pate has the advantage of a documentary history. But although Kirkman has been working in Kenya since 1948, he has found only five coins. In 1947, however, A. C. A. Wright brought a base silver coin into the British Museum which he had found at Warsheikh; it was of an Abu Bakr ibn Muhammad, and its design and type clearly belonged to those of the Kilwa coins. But there was no Abu Bakr of Kilwa; Pate, however, had a ruler of this name from 1494 to 1538. Warsheikh is far from Pate, but the possibility that Pate had a mint cannot be excluded. Far more important is that, while the Kilwa history stops at 1500, that of Pate continues up to the First World War. It bridges four more centuries which, in Tanganyika, we may only cover if the Mkwaja History can be obtained. An excavation of the palace quarter of Pate is thus a necessary complement to the excavation of the earlier cities of which I have spoken—for Pate was only founded in 1204.

In the foregoing, I have only been able to sketch some of the work of the past four years and to suggest some future projects. There is not a single field on which I have touched in which our knowledge is more than fragmentary. If at times I have seemed to present a series of problems rather than firm and new conclusions, it is because in East Africa one can hardly raise one's hand without striking

something new. It is not that there is too little information: there are so many problems, so much left to discover, that there is a lifetime of work for many men. If I have seemed to stress the importance of the archaeologist, I regard as of equal importance the need for the collection of historical traditions and information about the lives of the people. Dr. Whiteley's experiences at Tongoni suggest that there is a vast field open to the linguist, and I am certain that there is much poetry to be collected in the Mafia Islands. For the anthropologist, whom we need urgently to give us some interpretation of Swahili society, there is a virgin field.

### Addendum

Since this paper was delivered, Mr. E. C. Baker has most kindly given me some notes which he made at Mkwaja in 1923 and which include genealogies of the ruling family. Although they cannot replace the original document, they do afford some clue to the chronology of the numerous tombs of this family, which appear to date from c. 1600, and of which oral tradition has preserved the names of the occupants.

### Notes

<sup>1</sup> Lists of Sir John Gray's articles in *Tanganyika N. and R.* can be found in No. 35, July, 1953, and No. 42, March, 1956.

<sup>2</sup> J. S. Kirkman, 'The Excavations at Kilepwa,' *Antiq. J.*, 1952; *The Arab City of Gedi: Excavations at the Great Mosque*, 1954.

<sup>3</sup> Other articles by the Revd. Gervase Mathew, F.S.A., are: *The Times*, 26 June, 1950; *Report on the Sites of Songo Mnara Island*, February, 1952 (unpublished); 'Recent Discoveries in East African Archaeology,' *Antiquity*, 1953; 'Archaeological Remains on the Coast' in the *Tanganyika Handbook*, in course of publication.

<sup>4</sup> *MAN*, 1956, 61, ad fin.

<sup>5</sup> *The Prehistory of East Africa*, 1955.

<sup>6</sup> Edited by W. H. Schoff, New York, 1911, p. 28.

<sup>7</sup> C. Ptolemai *Geographi*, edited by C. Müller, 2 vols., Paris, 1883, 1901.

<sup>8</sup> Text in E. O. Winstedt, *The Christian Topography*, 1909.

<sup>9</sup> L.-M. Devic, *Le Pays des Zendjs*, Paris, 1883.

<sup>10</sup> G. Ferrand, *Relations de Voyages et Textes Géographiques*, Paris, 2 vols., 1913.

<sup>11</sup> Arabic text in S. A. Strong, 'History of Kilwa,' *J. R. Asiat. Soc.*, 1895; Portuguese text in J. de Barros, *Decadas da Asia*, Lisbon, 1552, edited by A. Baiao, Coimbra, 1931, pp. 308ff., 380f. I hope to publish complete translations in a work entitled *The Medieval History of the Tanganyika Coast*.

<sup>12</sup> The two versions of the Pate history are: C. H. Stigand, *The Land of Zinj*, 1913, pp. 28–102, English translation only; A. Werner, 'A History of Pate,' *J. Afr. Soc.*, 1914–15.

<sup>13</sup> E. Axelsson, *South-East Africa, 1488–1530, 1940*, summarizes the documents for this period and somewhat beyond.

<sup>14</sup> W. H. Ingrams, *Zanzibar, its History and People*, 1931, p. 517.

<sup>15</sup> E. C. Baker, 'Some Notes on the Shirazi,' *Tanganyika N. and R.*, No. 11, April, 1941, p. 2.

<sup>16</sup> *Loc. cit.*

<sup>17</sup> Sir John Gray, *Tanganyika N. and R.*, No. 38, 1955; R. Oliver, *Africa*, Vol. XXV, No. 3, July, 1955.

<sup>18</sup> E. Steere, *Swahili Tales*, 1888. W. Hichens, *al-Inkishafi*, 1939; *Diwani ya Muyaka bin Haji al-Ghassani*, Johannesburg, 1940; and, with A. Werner, *Utenzi ya Mwana Kuponu*, Medstead, 1934. See also C. Velten, *Prosa und Poesie der Suaheli*, Berlin, 1907.

<sup>19</sup> M. H. Dorman, 'The Kilwa Civilisation and the Kilwa Ruins,' *Tanganyika N. and R.*, No. 6, 1938; Songo Mnara: Mathew, *op. cit.* and in *Antiquity*, 1953; T. M. Revington, 'Some Notes on the



Mafia Island Group,' *Tanganyika N. and R.*, No. 1, 1936; D. W. I. Piggott, 'History of Mafia,' *ibid.*, No. 11, 1941; R. F. Stowell, 'Notes on Some Ruins at Tongoni, near Tanga,' *ibid.*, No. 4, 1937; N. Forster, 'A Note on Some Ruins near Bagamoyo,' *ibid.*, No. 3, 1937; F. Stuhlmann, *Beiträge zur Kulturgeschichte von Ostafrika*, Berlin, 1909, p. 843; Ingrams, *op. cit.*, p. 64.

<sup>20</sup> Gedi, pp. 129, 12-14.

<sup>21</sup> *The Book of Duarte Barbosa*, edited by M. L. Dames, Hakluyt Society, 1918, Vol. I, pp. 20, 22.

<sup>22</sup> Masudi, *Les Prairies d'Or*, text and translation edited by C. B. de Meynard and P. de Courteille, 9 vols., Paris, 1861 *seq.*; Vols. I, *passim*, and II, pp. 2-31, 44, give the tale of his adventures and knowledge.

<sup>23</sup> C. Defréméry and B. R. Sanguinetti, *Les Voyages d'Ibn Battoutah*, text and translation, 4 vols., Paris, 1862-; Vol. II, pp. 379f. give his account of his journey.

<sup>24</sup> Axelson, *op. cit.*, pp. 126f.

<sup>25</sup> I am grateful to Mr. W. Dick, Game Ranger, Morogoro, for helping me to make this estimate.

<sup>26</sup> Cf. Th. Nöldeke, *Sketches from Eastern History*, 1892, translated by J. S. Black.

<sup>27</sup> Cf. *History of the Imams and Seyyids of Oman*, edited by G. P. Badger, Hakluyt Society, 1871.

<sup>28</sup> Fr. Joao dos Santos, O. P., *Ethiopia Oriental*, Evora, 1609. The relevant passages are translated in *Hakluytus Posthumus or Purchas his Pilgrimes*, Vol. IX, Glasgow, 1905.

<sup>29</sup> J. J. L. Duyvendak, *China's Discovery of Africa*, 1949, pp. 14f.

<sup>30</sup> Cited by Duyvendak, *op. cit.*, p. 14.

<sup>31</sup> Cf. A. C. Hollis, *The Masai*, 1905, pp. 257, 317 and Plate XI.

<sup>32</sup> Sir R. Coupland, *East Africa and Its Invaders*, 1938, pp. 304f.

<sup>33</sup> The map is most easily found in A. Hourani, *Arab Seafarers in the Indian Ocean*, Princeton, 1953.

<sup>34</sup> I am grateful to Dr. J. Walker, F.S.A., for making this available to me.

<sup>35</sup> Cf. Sir John Gray, 'A Journey from Tete to Kilwa in 1616,' *Tanganyika N. and R.*, No. 25, 1948.

<sup>36</sup> See above, No. 23; Kirkman, *Gedi*, p. 155.

<sup>37</sup> 1931. Recently Miss Pelham-Johnson has most generously given her collection to the King George V Memorial Museum, Dar es Salaam. A description of it is shortly to be published by Dr. W. G. N. van der Sleen, of Amsterdam University.

<sup>38</sup> J. Walker, 'History and Coinage of the Sultans of Kilwa,' *Num. Chron.*, 1936; *id.*, 'Some New Coins from Kilwa,' *ibid.*, 1939; cf. G. S. P. Freeman-Grenville, 'A New Hoard and Some Unpublished Variants of the Sultans of Kilwa,' *ibid.*, 1954.

<sup>39</sup> See note 11.

<sup>40</sup> H. Mattingly, 'Coins from a Site-Find in British East Africa,' *Num. Chron.*, 1932.

<sup>41</sup> M. P. Charlesworth, 'A Roman Imperial Coin from Nairobi,' *Num. Chron.*, 1949.

<sup>42</sup> 1927, p. 45.

<sup>43</sup> F. B. Pearce, *Zanzibar, The Island Metropolis of Eastern Africa*, 1920, pp. 363-74.

<sup>44</sup> Cf. F. Stuhlmann, *op. cit.*, p. 860, note 2; Walker, *op. cit.*; Duyvendak, *op. cit.*, pp. 26f.

<sup>45</sup> Fr. Schjölth, *Chinese Currency*, 1929, Preface.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**The Yakan Cult Among the Lugbara.** By John Middleton, D.Phil. Summary of a communication to the Institute, 22 May, 1958

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The Lugbara are a Sudanic-speaking people, numbering a quarter of a million and living on the Nile-Congo watershed, in north-western Uganda and north-eastern Belgian Congo. They lack any traditional political authority except for the rudimentary authority of rainmakers and wealthy men known as 'men whose names are known.'

The cult of Yakan appeared first in Lugbara between about 1890 and 1905. This period was marked by the effects of the Mahdi revolt in the southern Sudan, of the Arab slave-raiding and of the expansion of the Azande empire. Also meningitis and rinderpest, together with a famine, appeared in Lugbara about 1895. Lugbara went to Rembe, a Kakwa (a tribe living to their immediate north), to obtain a magical water with which to combat meningitis and other diseases. The water was the same as that originally used by Dinka, Mundu and other neighbouring peoples to fight the Arabs, the Dervishes and the Azande. It was known to Europeans as 'Allah water.' Its use was widespread among the 'Nubis' who were with Emin Pasha and others, and who later mutinied in Uganda.

Those Lugbara who had obtained the water became important men, and on the setting-up of a Belgian administration in 1900, they were put forward by the Lugbara as 'chiefs.' The cult then petered out. The Belgians withdrew in 1908, and there was an interregnum until 1913, when the area became part of Uganda and an administration was set up again. Rembe and several assistants reappeared and travelled throughout much of Lugbara, dispensing the water and setting up an organization modelled on a European military one. There were grades of 'chiefs' of the cult, and adherents drilled with dummy rifles. In 1919 a large meeting was stopped by police, and there was a skirmish in which several people were killed. There were a few later fights, but the cult died out in the 1920s.

The drinking of the water was paid for with money. A drinker was promised that peace would come to the land and people; that they would be preserved from death; that their ancestors would come to life; that their dead cattle would come to life also; that they could flout government orders with impunity and need not pay tax; that they would be immune against Government rifles which would fire only water; and that they would soon obtain rifles to drive the Europeans from the country. Those who refused to drink the water would become termites when they died. However, it seems that pressure was brought upon all men and women to become cult adherents.

Rembe was said to come from God, and he had a snake oracle. The snake was known as *dede*, which was used also for the name of the cult. *Dede* means 'grandmother' in Lugbara, while the Kakwa for grandmother is Yakanye. These words are also used for small green snakes in which the soul of a grandmother may dwell, and the cult was said to protect its adherents as does a grandmother. The association with water was clearly to do with its use to prevent meningitis, which comes every dry season and ceases as soon as the rains appear.

Lugbara see their society as unchanging, and small redistributions of authority, within the family, occur within the framework of the traditional system and are made in terms of changes of authority within the cult of the dead. The introduction of new power into the total system is seen as coming from God, who created the world; only he can create new social relationships. Rembe the prophet, who is by now a mythical figure and so associated in Lugbara thought with God, outside the range of everyday social relations, was the agent by which it was made possible for Lugbara to respond to the new situation by themselves adopting and controlling this power. They did this by the principal adherents becoming 'chiefs' of the cult and later government-appointed chiefs.



## SHORTER NOTES

**Boulder-Chip Scrapers in the Eastern Arctic.** By Kaj Birket-Smith, Ph.D., D.Sc., *Etnografisk Samling, Nationalmuseet, Copenhagen. With a text figure*

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Coarse scrapers made of boulder chips are known from many parts of the world. W. H. Holmes adopted the Shoshone term *teshoa*, originally introduced by Leidy for describing these implements from Wyoming, and used it for characterizing similar artifacts from the coastal regions of Argentina.<sup>1</sup> In Alaska they seem to be extremely common in both Athapaskan and Eskimo sites. Their manufacture is thus described by Rainey:

'One of the native women at Gulkana [on the upper Copper River] obligingly made several for us in the following manner: a flat oval pebble selected from the beach was struck so that a thin, disc-like flake was detached; the edge of the flake was then battered against another stone to procure a blunt, retouched edge. In the upper Tanana dialect these tools are called *tei-tho*.'<sup>2</sup>

They have also been found in the interior of Alaska on the Tanana and on the central and lower Yukon,<sup>3</sup> and they are still in use for skin-cleaning not only by the Tanana but also by the Kutchin, Tanaina and Ingalik.<sup>4</sup> Examples of the same primitive type occur sporadically at least as far south as southern California and the Great Basin,<sup>5</sup> but here they sometimes seem to be hafted.<sup>6</sup>

Simple boulder chips apparently intended for skin-scrapers are likewise known from many Alaskan Eskimo sites. Thus, Frederica de Laguna writes of her excavations in Kachemak Bay, Cook Inlet:

'The chips are oval in outline, but with considerable variations in size and proportions. . . . The majority range between 8 and 11 centimetres in length. The natural rounded surface of the boulder forms one side and meets the fractured surface sharply at one edge to form a curving blade, and bluntly at the opposite edge to form a grip. Some of the chips have been intentionally retouched along one edge or all the way around . . . ; others have the edges blunted and battered by use . . . ; on others it is worn smooth. . . . Some chips . . . are worn not only on the edges but on the sides. . . . These boulder chips were evidently not hafted, and could have been used as knives, scrapers, wedges, choppers for splitting bones for marrow, saws for wood, and hammers for notching stones. No doubt a single specimen did serve a variety of uses. Boulder chips were found at every site where excavations were undertaken, and belong to all stages of the Kachemak Bay Eskimo culture and to the pre-Russian Indian culture.'<sup>7</sup>

In Prince William Sound, on the other hand, only four specimens were found during our joint excavations in 1933, 'in striking contrast to the hundreds discovered in Kachemak Bay from every site and every culture stage.'<sup>8</sup> Two of them, now in the Danish National Museum (P 726 and P 990), are illustrated here. In addition, they are described from Kodiak Island,<sup>9</sup> Hooper Bay,<sup>10</sup> and Point Barrow,<sup>11</sup> as well as from inland Eskimo sites on the Kobuk River.<sup>12</sup> Similar implements made of slate (?) have been found in Eskimo sites on Chetyrekhtolbovoi Island off the mouth of the Kolyma on the north coast of Siberia, according to N. A. Beregovaya (in *Sovetsk. Arkheol.*, Vol. XX., Moscow, 1954, p. 306, cf. Plate III, figs. 1-3).

So far very few examples of this type are known from the eastern Arctic. From Port Burwell and Nuvuk Island, Labrador, Dr. de Laguna mentions 'a scraper made of a slate slab and another of a boulder chip.'<sup>13</sup> Among the scrapers discovered by Wintenberg in Dorset sites in Newfoundland 'the most simple kind . . . consists of irregularly shaped chips with one or more

edges trimmed to a bevel for use . . . ; one has the edge formed by retouching from both faces.'<sup>14</sup> As far as can be judged by this very summary description, none of them are true boulder chips. However, Wintenberg also refers to a 'crude adz-like object,' which actually may be a boulder-chip scraper.

'It is derived from a spall broken from a boulder of hard, light grey rock, 1 3/8 inches thick. Only the waterworn surface shows marks of chipping, but the broken face of the spall has been dressed into shape and partly smoothed. The cutting edge is fairly sharp. A spall from a boulder of dark grey rock from the Cow Head site (Cat. No. VIII-A-365), 5 inches long, 3 1/8 inches wide and about 1 1/2 inches thick at the upper end, thins down to an almost sharp edge at the lower end, and may have been intended for a similar blade. It retains the waterworn surface of the rock on one face and has been trimmed into shape on the upper end of the broken face. The thin edge looks as if it had been worn by use, but this may be due to sand erosion.'<sup>15</sup>

It is possible that some objects from the Hopedale area, Labrador, should also be interpreted as boulder-chip scrapers.<sup>16</sup>

In the discussion of these implements it seems to have passed unnoticed, however, that they occur in West Greenland, where in the Egedesminde District I found them still in use as late as 1918. They were employed for scraping seal skin in order to remove the subcutaneous tissue. The native term was *uiliissaq*, i.e. something resembling a mussel, the same word which designates a cup-shaped wooden scraper and referring to the use of mussel

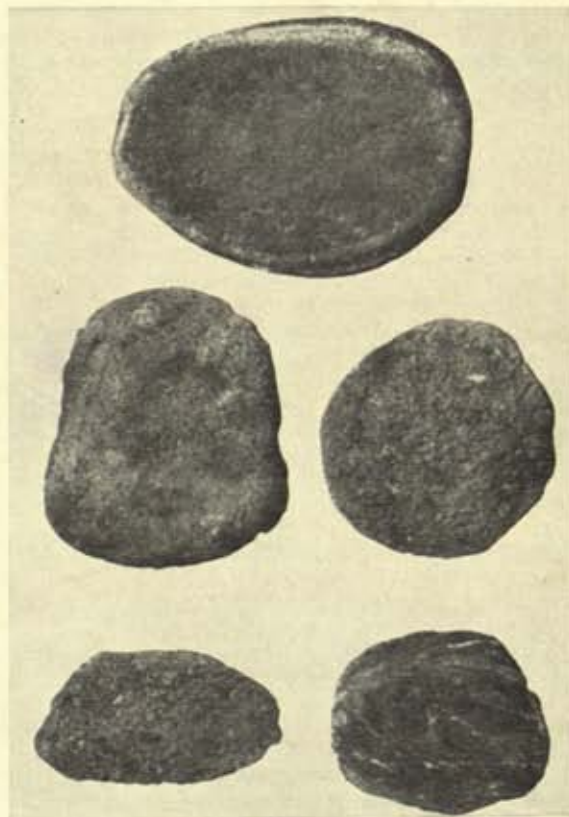


FIG. 1. ESKIMO BOULDER-CHIP SCRAPERS

Top: from Qipingassoo, Egedesminde District, West Greenland (length 12.1 centimetres). Middle row: from Comer's Midden, Thule District, Greenland, and Iginiarfik, Egedesminde District, West Greenland. Bottom row: from Palugvik, Prince William Sound, Alaska. National Museum, Copenhagen



shells for the same purpose. It is quite possible, however, that this is not the original term, not only because there is very little resemblance in shape between boulder-chip and cup-shaped scrapers, but also because the latter is a Thule Culture type<sup>17</sup> and therefore much later than the primitive stone implements at least in Alaska. I brought back two specimens, which are now in the National Museum<sup>18</sup>:

L 7920, from Qipingassoq. An oval chip of dark rock, one face of which is the original surface of the boulder. Thick edges with only slight traces of retouching. 12.1 by 8.8 centimetres.

L 7921, from Iginiarfik. A sub-circular gneiss chip. Slight retouching along most of the periphery. Diameter about 8 centimetres.

Besides these I know of only one other specimen among the many thousands of archaeological items from Greenland in the National Museum, viz. L 3: 11286, found in the top layer of the so-called Comer's Midden at Thule and previously illustrated by Holtved.<sup>19</sup> It is made of greyish rock, approximately trapezoidal with round corners and two concave edges, one of which shows slight traces of retouching. It measures 9.5 by 8.2 centimetres.

It is indeed a remarkable fact that boulder-chip scrapers seem to be so rare east of Alaska. One reason for this is, perhaps, that they have been more or less replaced by simple slabs of slate such as are extremely common in many finds from Greenland<sup>20</sup> and apparently also from Labrador.<sup>21</sup> There is also, of course, the possibility that on account of their insignificant appearance they may sometimes have been overlooked, although the chance is but small in consideration of how many trained archaeologists have been working in both Arctic Canada and Greenland. However, the possibility cannot be wholly ignored, and that is the reason why these lines have been written.

#### Notes

<sup>1</sup> W. H. Holmes, 'Stone Implements of the Argentine Littoral' (in A. Hrdlička, 'Early Man in South America,' *Bur. Amer. Ethnol. Bull.* 52, Washington, 1912), p. 131; *id.*, 'Handbook of Aboriginal American Antiquities,' *Bur. Amer. Ethnol. Bull.* 60, Washington, 1919, Part I, p. 302.

<sup>2</sup> Froelich G. Rainey, 'Archaeology in Central Alaska,' *Anthrop. Pap. Amer. Mus. Nat. Hist.*, Vol. XXXVI, Part 4, New York, 1929, p. 360.

<sup>3</sup> *Ibid.*, pp. 365f., 378; H. W. Krieger, 'Archeology of Central Alaska' (in A. Hrdlička, 'Anthropological Survey in Alaska,' 46th *Ann. Rep. Bur. Amer. Ethnol.*, Washington, 1930), p. 146; Frederica de Laguna, 'The Prehistory of Northern North America as seen from the Yukon,' *Mem. Soc. Amer. Archaeol.*, Vol. III, Menasha, 1947, pp. 128, 150.

<sup>4</sup> Cornelius Osgood, 'Contributions to the Ethnography of the Kutchin,' *Yale Univ. Publ. Anthropol.*, Vol. XIV, New Haven, 1936, p. 70; *id.*, 'The Ethnography of the Tanaina,' *ibid.*, 1937, p. 103; *id.*, 'Ingalik Material Culture,' *ibid.*, 1940, p. 81.

<sup>5</sup> de Laguna, *op. cit.*, p. 189. However, the author does not distinguish between boulder-chip and *ulu*-like scrapers.

<sup>6</sup> A. G. Morice, 'Notes Archaeological, Industrial and Sociological on the Western Dénés,' *Trans. Canad. Inst.*, Vol. IV, Toronto, 1895, pp. 50f.; John J. Honigsmann, 'The Kaska Indians,' *Yale Univ. Publ. Anthropol.*, Vol. LI, New Haven, 1954, p. 27.

<sup>7</sup> Frederica de Laguna, *The Archaeology of Cook Inlet, Alaska*, Philadelphia, 1934, p. 61.

<sup>8</sup> *Ead.*, 'Chugach Prehistory,' *Univ. Wash. Publ. Anthropol.*, Vol. XIII, Seattle, 1956 p. 133.

<sup>9</sup> Robert F. Heizer, 'Archaeology of the Uyak Site, Kodiak Island, Alaska,' *Anthrop. Records*, Vol. XVII, Part 1, Berkeley and Los Angeles, 1956, p. 43.

<sup>10</sup> Wendell Oswalt, 'The Archaeology of Hooper Bay Village,

Alaska,' *Anthrop. Pap. Univ. Alaska*, Vol. I, Part 1, College, 1952, p. 58.

<sup>11</sup> de Laguna, *Prehist. N. Amer.*, p. 187.

<sup>12</sup> J. L. Giddings, 'The Arctic Woodland Culture of the Kobuk River,' *Mus. Monogr.*, Philadelphia, 1952, pp. 80f.

<sup>13</sup> de Laguna, *Prehist. N. Amer.*, p. 189.

<sup>14</sup> W. J. Wintemberg, 'Eskimo Sites of the Dorset Culture in Newfoundland,' *Amer. Antiq.*, Vol. V, Menasha, 1939-40, p. 99.

<sup>15</sup> *Ibid.*, p. 313f.

<sup>16</sup> Junius B. Bird, 'Archaeology of the Hopedale Area, Labrador,' *Anthrop. Pap. Amer. Mus. Nat. Hist.*, Vol. XXXIX, Part 2, New York, 1945, p. 167.

<sup>17</sup> Therkel Mathiassen, 'Archaeology of the Central Eskimos,' *Rep. Fifth Thule Exped. IV*, Copenhagen, 1927, Vol. II, p. 91. The original term was possibly *ersorsit*, 'stone to prepare skins with,' cf. Schultz-Lorentzen, 'Dictionary of the West Greenland Eskimo Language,' *Medd. om Grønl.*, Vol. LXIX, Copenhagen, 1927, p. 48.

<sup>18</sup> Kaj Birket-Smith, 'Ethnography of the Egedesminde District,' *Medd. om Grønl.*, Vol. LXVI, Copenhagen, 1924, p. 98, cf. fig. 70c-d.

<sup>19</sup> Erik Holtved, 'Archaeological Investigations in the Thule District,' *Medd. om Grønl.*, Vol. CXLI, Part I, Copenhagen, 1944, Plate XXV, fig. 13, cf. p. 256.

<sup>20</sup> Therkel Mathiassen, 'Inugsuk,' *Medd. om Grønl.*, Vol. LXXVII, Copenhagen, 1930, pp. 221f.; *id.*, 'Contributions to the Archaeology of Disko Bay,' *ibid.*, Vol. XCIII, Part 2, 1934, p. 107; Holtved, *op. cit.*, p. 256; Birket-Smith, *op. cit.*, pp. 54, 98f.

<sup>21</sup> Cf. Bird, *op. cit.*, pp. 140, 167.

#### Guy Fawkes Day at Fresh Creek, Andros Island, Bahamas.

By Daniel J. Crowley, Ph.D., Department of Anthropology, Northwestern University, Evanston, Illinois

## 158

The Guy Fawkes celebration on 6 November, 1957, at Fresh Creek, Bahamas, was announced by a small boy who shouted to the handful of out-of-season tourists and yachtsmen, 'We go burn a guy tonight!' Further investigation<sup>1</sup> disclosed, not capital punishment or human sacrifice, but a traditional Guy Fawkes celebration including the burning of an effigy. The date had been changed from 5 November because a prominent local person had died that day, and his wake or 'settin-up' had occupied the night, and his funeral and band procession the following morning.

Andros Island, although 1,600 square miles in area and only 100 miles east of Miami, has a population of less than 9,000, and its marshy interior is still largely unexplored. On the north shore of a wide, tidal 'Fresh Creek' on the east coast of Andros is the village of Coakley Town with a population of 500, almost all of African origin. On the south shore of the creek is the Andros Town Yacht Club and Hotel, a luxurious establishment developed by Mr. Axel Wenner-Gren, and catering to wealthy British, Canadian and U.S. tourists, bonefishermen, and yachtsmen. Behind the hotel there are extensive housing units, apartments, and barracks for the permanent staff, most of whom derive from the other coastal settlements of Andros Island. It was members of this hotel staff who participated in the Guy Fawkes celebration.

At about 6 p.m. a dancing procession of men and boys carrying the effigy paraded along the docks, stopped at several yachts, proceeded through the hotel grounds, and then returned to the Flamingo (pronounced 'Filimingo') Club, a small night club catering to the staff and the local people. During the procession the dancers 'capered' and did 'the jumpin dance,' the former a kind of strutting or cake walk, the latter a leaping arabesque with a wide range of individual variation. One man played a small drum made from a commercial nail keg over the end of which had been stretched a sheepskin. Another played a drum made from the top of a small barrel with the metal hoops still in place, and the staves painted alternately red, white, and blue. Both drums were termed 'John Canoe' or 'Junkanoo' drums, from the Christmas mas-



querade,<sup>2</sup> and both were carried under the left arm suspended from the neck by a cord, and beaten with the fingers and palms of both hands. The sheepskins were looser, and hence the tone lower than is usual in West Indian drumming. Cowbells are the traditional accompaniment of John Canoe drumming, but since no cowbells could be found in the settlement, four boys had made rattles by inserting pebbles into used beer tins. These were made to approximate the sound of cowbells by the players muffling them with their hands while shaking them. Two small boys also accompanied the drum rhythms by blowing short and long blasts on home-made reed whistles. There was no attempt at costumes or masking among the dancers or musicians, except that several had bound kerchiefs around their temples 'like pirates.'

When the procession arrived at the Flamingo Club, a lengthy discussion was held during which it was decided to postpone the burning of the effigy until after the showing of a film, an old American 'western,' by the club management. As soon as the film was over, a bonfire was lit outside the club, and the sheepskin heads of the two drums were heated to increase their tension and thus produce a higher, more vibrant tone. The procession once more went its way through the staff quarters, and followed a gravel path to the shore of a small pond. Here the effigy was set up while the dancers performed before it. The effigy was made from a ragged but clean suit of 'coveralls,' white duck trousers and jacket made in one piece and fastened up the front similar to Winston Churchill's 'blitz suit.' It was stuffed tightly with dry straw, and the same material was bound with cord to make a 'head.' The 'face' was a piece of stiff white cloth on which were crudely drawn a red mouth and two black eyes and eyebrows. The top and sides of the head were covered with a white kerchief tied under the 'chin.' The effigy was fastened to a T-shaped structure of wood, the arms outstretched, and the base of the T was sharpened so that it could be forced into the ground to make the effigy stand upright.

The dancing continued only a short time before the effigy was lighted, making a spectacular fire reflecting in the pond. The dancers and musicians continued throughout the burning, but a few minutes afterwards began their procession back towards the village. On their arrival at the barracks, they were joined by several other dancers, including a very obese young woman called Josie. While some of the spectators continued home, the remainder formed a circle around the dancers in the middle of the white crushed-rock road. They began to sing as one or sometimes two dancers performed in the middle of the ring. These dancers sought to outdo one another in the intricacy and agility of their steps, and

there was frequent goodnatured laughter when a dancer faltered or stumbled. A certain amount of innuendo developed around the person of Josie, the only woman dancing, and most of the dance steps were directed to her. A typical step consisted of arching the body with shoulders forward and hands behind the head. Then simultaneously the dancer stood on tiptoes and shifted his pelvis forward and upward in an exaggerated version of what Americans term 'the booms.' The step is very widely known in the West Indies.<sup>3</sup>

Songs were chanted or sung in the 'deep' Andros dialect of English, and were familiar and comprehensible to the bystanders, but incomprehensible to speakers of standard English. They were one or two lines in length, and highly allusive in subject matter.<sup>4</sup> Several had short two-and-three-word 'choruses' which were enthusiastically shouted out by the bystanders. Examples are:

Oh Gal, Don't do that thing! Hold Y'self!  
Old Man, Hold your dog! Dog going bite me!  
Bow wow wow!  
Josie, Take you hand from my pocket! Josie lick!  
High in my gaulding [heron or egret, *Ardeidae*]  
Half mast!  
Side of man-of-war! Fall overboard! Over man-of-war!  
Man take one to satisfy; Woman take two to make move!

When questioned about the significance of the effigy of Guy Fawkes, most of the spectators were vague. 'It something we having from England, mon.' or 'Burning a guy is like John Canoe.' However, one young man called Capron who had helped to make the effigy and was perhaps the most skilful of the dancers answered, 'It something we have from the Loyalists [repatriated to the Bahamas in 1783 after the American Revolution]. A man name Guy Fawkes try to blow up Parliament in 1583, I think it was [1605]. So we making a kind of a masquerade out of it here. The dancing and drums we get from the old people from Africa.'

#### Notes

<sup>1</sup> Thanks are due to Mr. Kenneth S. Keyes, Jr., of Coral Gables, Florida, for making possible the research on which this paper is based.

<sup>2</sup> Ira De A. Reid, 'The John Canoe Festival,' *Phylon*, 1942, Part 4, pp. 349-370.

<sup>3</sup> Daniel J. Crowley, 'Festivals of the Calendar in St. Lucia,' *Caribbean Quarterly*, Vol. IV, No. 2 (December, 1955), pp. 120f.

<sup>4</sup> *Loc. cit.*

## CORRESPONDENCE

### Garó Cross-Cousin Marriage. Cf. MAN, 1958, 2

159 SIR,—Miss Nakane clears up a good deal of the obscurity which was left by Playfair, Bose, and Mukherjee in their discussions of Garó cross-cousin marriage, and of the relationship of lineages to one another. Much of her article outlines very well the organization of marriage relationships among the Garó, but she makes a few statements which, for the sake of the record, should be corrected. Since I spent nearly two years recently studying the social organization of the Garó, I would like to do so. It is possible that Miss Nakane's data refer to an area with slightly different organization from the areas in which I worked. However, she did much of her work in a region not far from that which I knew best, and it seems unlikely that the differences are great. I believe, then, that Miss Nakane's data need correction on the following points:

(1) She states that the ideal arrangement for the Garó exists when two villages exchange husbands so that the sons and brothers of the women of one village move to become the husbands of the women of the opposite village. In my experience, the husbands of the women of a village lineage come from a large number of lineages from many other villages, and the sons and brothers of a single lineage generally move out to a similarly large number of villages, and I never encountered suggestions that this was contrary to any ideal. Men acquire rights to cultivate in a particular village only by virtue of marrying a woman of the village and not simply by being born into a lineage whose members have the specific right to marry into another village and lineage.

(2) Miss Nakane mentions several times that a man may not marry his sister's husband's sister. I knew of several cases where this had in fact happened, i.e. two men had married each other's sisters.



I could elicit no expressions of disapproval about this situation, although I inquired extensively. They did recognize the relationship as a special one. The two men have a peculiarly symmetrical relationship to each other, which is certainly not typical, but is not wrong. There is even a special phrase to describe it: *paping deping*, with a meaning something like 'father and son to each other.'

(3) She says that the *nokma* of satellite villages belong to the same lineage as the principal village *nokma*. Most villages have more than one man who is recognized as *nokma*, though one of them is always recognized as more important than the others. The secondary *nokma* do not necessarily come from the same lineage as the first, and frequently are from the opposite moiety. Similarly, the first *nokma* of satellite villages frequently come from different lineages, and sometimes from the opposite moieties.

(4) The author states that each *nok* has two *nokchame* relationships. In fact each *nok* may have several. The several sons of a single *nok* almost always marry into different *nok* of widely different lineages, generally into separate villages. The men who marry in (just one in each generation) may come from the same *nok*, but are more often from different, though closely related, *nok*.

(5) She mentions the occasional patrilocality as if this were a disturbing variation in the generally uniform matrilocality. Patrilocality is in fact an absolutely essential arrangement for a minority of the couples in this society: some men must bring their wives to live in their villages, though never into their own mother's house. If this did not happen all the adult married men of the village would be from other lineages than that of the women, and since women depend upon male members of their own lineage for certain things, this is felt to be an impossible situation. Patrilocality is not, therefore, to be dismissed as a variation from the traditional pattern; it is rather an integral part of the system and must be taken into consideration in any analysis of it.

(6) In my experience, though wet-rice-cultivation has had profound effects in many aspects of the culture, it has not yet greatly affected the marriage system. People who have had wet rice for two or three generations still appoint *nokkrom*, and still maintain most of the kinship relations of hill-rice-cultivators. What wet-rice-cultivation does accomplish is to concentrate wealth into the hands of the few people who have the foresight to claim the land. This may eventually affect the kinship system profoundly, but as yet there has seldom been any disruption, and in particular, tendencies towards patrilineality are exceedingly rare.

(7) In note 17 she says that *chowary* means 'son-in-law' in general, rather than being limited to non-*nokkrom*. Bose was in fact right in his definition, since in actual usage, the term *chowary* is never applied to a *nokkrom*, though Garo interpreters, whose English is apt to be less than perfect and who tend to oversimplify in the matching of English and Garo equivalents, might easily make some such statement as '*chowary* just means "son-in-law," but *nokkrom* means "heir."'

When Miss Nakane considers the *nok*, the contrast between the *nokna* (heir) and other daughters, the actual workings of Garo cross-cousin marriage and in particular the realities of classificatory versus first-cross-cousin marriage, she is on firmer ground. Here her discussion is accurate and clear.

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#### A Trobriand Medusa? Cf. MAN, 1958, 65

**160** SIR,—Further to Dr. R. M. Berndt's re-interpretation of the stylized design on a Trobriand Island shield, the following description of the way in which Kiwai natives decorated the male figures carved on the tops of the posts in their long houses (from Dr. G. Landtman's 'Papuan Magic in the

Building of Houses,' in *Acta Academiae Aboensis, Humaniora*, Vol. I, Part 5, 1920, p. 21) seems relevant enough to warrant particular mention. We are told that: 'Round the neck are suspended the claws of a "wario" and "ruburubu," two hawks, and also male and female genital organs. The latter, inserted into each other, have in war the effect of causing the enemy to be so much occupied among themselves that they will be caught unawares by the attacking party.'

A snag in accepting this interpretation for the shield design is Malinowski's insistence that the Trobrianders 'never fought without warning' (MAN, 1920, 5). He describes a type of organized warfare with pitched battles. Raiding seems, however, to have occurred at times of crisis, e.g. during a famine, as described by Malinowski's informant Tokulubakiki. Malinowski says that 'very seldom, and only in the case of very brave and distinguished warriors, were the shields painted.' If their function was to shame the enemy by means of visual abuse, and ridicule them by emphasizing the incompatibility of war and sex (which is Dr. Berndt's interpretation) why were so very few painted? Malinowski mentions two or three for a whole war.

Perhaps there is more to it. Describing war magic, Malinowski says that 'the magician "coram publico" chanted over the shields so as to impart to them the power of warding off all spears.' Thus we see that all the shields were given some magical power which they would exercise 'per se' in battle. May this be tied in with Landtman's data? The hypothesis would then be that the painted shields were especially powerful (like their owners), being able to weaken the enemy's resistance by preoccupying them with sex. The design would thus be intended to work whether the enemy saw it or not (cf. Dr. Berndt's explanation). This whole interpretation falls down if Dr. Landtman's description of the Papuan material is inaccurate, or if the same ideology does not exist in the Trobriands, or if the shield design does not represent the human sexual act.

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VERNON REYNOLDS

#### On 'Legalized Incestuous Marriage'

**161**

SIR,—I would question the propriety of the phrase 'legalized incestuous marriage' on p. 114 of the sixth edition of *Notes and Queries on Anthropology* (1951).

Incest is by definition a crime (Oxford English Dictionary). And *Notes and Queries* defines incest as a prohibited act or relationship (p. 113).

Incest is not, of course, synonymous with biological inbreeding. In some societies it would be incestuous to marry a member of one's own clan, or a parallel cousin, even though she be only a sixty-fourth cousin, or so remotely related that the actual genealogical relationship could not be determined for lack of specific information, while, on the other hand, marriage with a first cross cousin may be not only permitted but required. Marriage between brother and sister, which is permitted to ruling families in some cultures, is, I submit, not 'legalized incest,' which is a contradiction of terms, nor is it incest at all. It is an instance of endogamy, i.e. marriage permitted or required within a specified group, in this case the family.

In his notable essay, 'On a Method of Investigating the Development of Institutions; Applied to Laws of Marriage and Descent' (*J. Anthropol. Inst.*, Vol. XVIII, 1888, pp. 245-69), Tylor gave us a key to an understanding of the origin of incest taboos. I developed his thesis in 'The Definition and Prohibition of Incest' (*Amer. Anthropol.*, Vol. I, 1948, pp. 416-35; reprinted in White, *The Science of Culture*, 1949), and have therein commented upon such phrases as 'sanctioned incest' (pp. 429 and 322, respectively, in the two printings cited).

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## REVIEWS

## GENERAL

**Social Theory and Social Structure.** By Robert K. Merton. Revised ed. Glencoe, Ill. (Free Press), 1957. Pp. xviii, 645. Price \$7.50.

162

The core of this volume was published under the same title in 1949 and the work has been influential and widely read. As the first edition was not reviewed in this journal, a brief summary of its contents may not be out of place. Of the 15 chapters all but one had been previously published and represented much of the work from which the author had gained his deservedly high reputation.

Part I consisted of a very useful statement of the structural-functional approach and also two papers on theory and method; the essay on 'Manifest and latent function' must still be considered essential reading for all interested in this theoretical position. Part II discussed the concept of anomie, which for Merton centres round the problem of how cultural structure generates pressures for socially deviant behaviour because of discrepancies between culturally prescribed aspirations and social avenues for realizing these. In addition it contained a discussion of the formal organization of bureaucracy. Part III, entitled 'The Sociology of Knowledge and Mass Communication,' included essays on the Radio and Film Propaganda studies carried out jointly with Lazarsfeld. Part IV, 'Studies in the Sociology of Science,' comprised five chapters which amongst other things treated contemporary relations between science and society.

The new edition greatly enlarges the book: new chapters nearly equal the size of the original volume. It cannot claim to be easy reading. The style is an interesting combination of lucidity and verbal extravagance, but the reading is not dull; on the contrary, it is absorbing. The new chapters reflect themes clearly stated in the first edition. Merton has emphasized the need for middle-range theories which lie between minor research hypotheses and 'the all-inclusive speculations comprising master conceptual schemes.' He has also emphasized the need for the progressive codification of theory and procedures of analysis. Both views are reflected in the additions. Perhaps of most interest are the two chapters on reference groups, one of which was previously published in collaboration with Alice S. Rossi. The currently favoured concept of reference group is based on the observation that individuals frequently orient themselves to 'groups' other than their own in shaping their behaviour and values. The first paper originally came from a symposium discussing the 'American Soldier' volumes and drew evidence from the various attitude questionnaires sent to Second World War conscripts. Of particular importance was the *post hoc* use of the concept of 'relative deprivation' to clarify many puzzling features in the results. The professed object of the second and longer paper is to summarize research and theoretical advance in the last six years. Essential clarifications are made, such as the distinction of two major types of reference groups: the 'normative' and 'comparison' types. The chapter has the hallmark of a working notebook but it provides a very competent and interesting review of papers produced in recent years and offers numerous stimulating research leads.

There is a chapter with similar aims entitled 'Continuities in the Theory of Social Structure and Anomie' and also a study of personal influence in a small American community.

Much of the 1949 edition has been of direct interest to anthropologists. This new edition deals with concepts not unmet in their contemporary discussions. To these it is an admirable introduction and stimulus. The book as a whole must unquestionably still hold its place as important reading for all interested in sociology and social research.

GEORGE W. BROWN

**Studies in Applied Anthropology.** By L. P. Mair. London Sch. of Econ. Monog. on Soc. Anthropol., No. 16. London (Athlone Press), 1957. Pp. 81. Price 13s. 6d.

163

This is a volume of eight essays by the Reader in Colonial Administration in the University of London on various aspects of social change and administrative development in non-

Western societies. Two are on the place of the anthropologist in the development of 'colonial' territories, one is on economic individualism in African society, one on African chieftainship, one on education, two on African land tenure, and one on government in self-governing territories. Six of these are reprints of earlier-published papers.

The two essays on the role of the anthropologist provide the main theme of the book. Dr. Mair considers the difficulties facing the anthropologist, how he can best analyse social problems with which he is himself personally concerned—both because of what he learns from his research and also, perhaps, because of the sort of person he is who is likely to want to study human society in the first place. She discusses why the 'practical' man tends to ignore the advice and experience of anthropologists, especially when they do not suit his already chosen policies; and she points out that the anthropologist often forgets that the administrator is as much bound by his own customary values as are those whom he administers. The anthropologist cannot decide upon policy, but can only supply information on specific courses of action by the administrator, who is the person responsible for selecting them. The anthropologist should be consulted in these matters because it is only he who sees society as being something other than a mere aggregate of individuals whose attitudes and behaviour the administrator wants to change, and it is only he who really knows anything about social integration. Until people realize that there is such a thing as integration—although they can recognize disintegration when they see it—they will not regard the advice of anthropologists as being necessary, whether it adds support to their policies or not.

The papers on economic individualism and on land tenure state the opinions of the anthropologist with pleasingly unanthropological clarity. The author draws much on her experience of the Baganda and avoids too much generalization about 'the African.' The essays on chieftainship (1936) and education (1935) are succinct but rather dated and obvious to the anthropologist today. This is perhaps a measure of advances in thinking of these matters that have been made over the last 20 years.

Dr. Mair shows that in all these problems, almost all surrounded by prejudice and ignorance, there are usually two sides, and she stresses their moral and 'practical' complexities. The arrogant and irresponsible attitudes of some recent 'social engineers' and possessors of 'know-how' of developing the undeveloped are shown up very clearly in contrast with Dr. Mair's more educated and more knowledgeable analysis of these essentially moral issues. It will do many people good to read her essays.

JOHN MIDDLETON

**Custom and Conflict.** By Max Gluckman. Oxford (Blackwell), 1957. Pp. x, 173. Price 12s. 6d.

164

Under the title *Custom and Conflict*, Gluckman gives us the text of six very successful talks given on the Third Programme in the spring of 1957. The central theme of these broadcasts is an apparent paradox, viz. that social conflicts, whether they take the form of feuds, rebellions, family estrangements, or ritual expressions of hostility, promote the cohesion of the society in which they occur. They do not destroy it, as we might expect. Each chapter has a paradox for a title. 'The Peace of the Feud' contains an analysis of Nuer material, and Gluckman presents the now familiar thesis that agnatic descent groups which are hostile to each other yet follow rules when they feud, and unite against a common foe. This 'ordered anarchy,' according to Evans-Pritchard, forms the basis of tribal cohesion in an acephalous society of the Nuer type. Gluckman here carries the argument a stage further, and points out, in an interesting passage, that loyalty to the agnatic feud group may conflict with the obligations a man has to his in-laws, who necessarily belong to other lineages, and also with his duties to the members of his local group or to his material kin. 'Feud is waged, vengeance taken when the parties live sufficiently far apart. . . . But where they are close together, many institutions and ties spread to exert pressure on the quarrellers to reach a settlement' (p. 19).



The agnate-affine conflict is again stressed in a chapter headed 'Estrangement in the Family' in which the taboos governing marriage and parental relations are described as due to the need to make artificial barriers between husband and wife, or father and son, in the interests of the wider agnatic lineage or clan group. Such conflicts, and the mechanisms by which they are dealt with, also act, according to Gluckman, to strengthen the whole society by the many links they forge between individual and individual, group and group.

The paradox of 'Frailty in Authority' is the subject of the third, and perhaps the most interesting talk. Here Gluckman contrasts the high virtues expected of a king, and especially of a divine king, with his inevitable human weaknesses. The ruler walks the tight rope between the interests of one group and another, between the exercise of one kingly virtue and that of another mutually exclusive one. Hence authority is inevitably a source of disappointment to those who accept it, and popular princes become hated kings. Political structures themselves also consist of groups with conflicting interests, and princes with rival claims. When a rebellion takes place, the warring elements unite to put another prince on the throne, and the belief in kingship is renewed. Conflict has brought cohesion in the long run, if not in the short. The same hypothesis underlies Gluckman's treatment of witchcraft and ritual and, in particular, his interpretation of rites of rebellion or of reversal. In a last and ambitious chapter, he considers the phenomenon of the colour bar in South Africa as a series of conflicts which bind together the distinct ethnic groups of which the nation is composed.

Gluckman's suggestions are, as always, stimulating, and he handles his material most skilfully within the limits of a series of short talks. The book is very readable and persuasive and gives sufficient illustrative matter to drive home the theoretical points. There are, however, major difficulties if his thesis is to be taken *au pied de la lettre*. To begin with, his conception of conflict is in essence a psychological one, or even a psycho-analytic one. Part of his explanation of rebellion is in terms of universal attitudes of ambivalence towards authority, and part in the form of a structural analysis of the institution of chieftainship, and Gluckman slides from one plane to the other. His treatment of authority surely does not become sociological, as distinct from psychological, by the mere avoidance of such terms as Oedipus complex. His account of the anxious king, the frail authority, is expressed in almost Shakespearean language as an inner conflict and the illustrative quotation, perhaps significantly, comes from 'Macbeth' and not from Gluckman's Lozi notebooks. I do not myself observe the psychology taboo which many anthropologists now maintain, but I believe it to be essential to distinguish clearly between individual conflicts, conscious or unconscious, and social groups with opposing interests.

But there is a more fundamental question to ask. Why must these facts be explained in terms of conflict? Gluckman acknowledges the strength of the bonds which unite men to all their different categories of kinsmen, whether agnates, affines, or matrilin. He emphasizes the network of ties which link the different feud groups in a segmentary society. He analyses the balance of interests which result in the acceptance of one man as king. Would it not be simpler to add that men value these ties and make them for positive reasons, even if some of them conflict, and that, in any case, societies usually have mechanisms for dealing with tensions of this sort, and that these are usually more or less successful?

It is true, of course, that kingship in most primitive societies has to be constantly maintained against the interests of the different groups into which the tribe could easily split, since rulers of this type are usually without sufficient executive staff, standing armies or adequate economic pre-eminence. Hence, as Gluckman explains in some stimulating pages, the king tends to bolster up his precarious position by a conscious policy of making alliances, delegating authority, putting the blame on subordinates and also retiring behind the veil of ritual. It is surely unnecessary to conclude from this that rebellion is inevitable and that it actually strengthens the kingship. In fact the history of Bantu Africa is a story of broken empires, split kingdoms and monarchs who failed to 'make the grade.' Some rebellions may result in putting a new king on the throne with temporarily strengthened powers, yet the position of such a mon-

arch seems to me to be nothing like as secure as that of a Bantu king who has overcome a series of rebellions against him and who has killed the most powerful of the potential rebels! Many rituals of kingship recognize this fact. There are some ceremonies which Bemba kings only carry out when they have been on the throne some time and consider that they are strong enough. I believe that the concept of kingship as a precarious art in which the ruler gathers strength through personality and the skilful winning of support is common in Bantu society. The authorities who have succeeded in this process cease to be frail, or presumably to feel frail.

Gluckman's plea for the long-term benefits of conflict represents perhaps another turn of the anthropological whirligig. In the thirties, British anthropologists trained by Malinowski concentrated on the positive functions of the family and its extensions to form larger groups which would permit wider co-operation. Hence our absorption in the study of primary relationships within the family, which of course often involved conflicts; Malinowski's analysis of the father-mother's brother and the cross-cousin conflicts in a matrilineal society initiated a new type of work in this field. We were not then so much concerned with the 'total social structure,' to use Radcliffe-Brown's term.

In the forties the return to Durkheim led to a concentration on the formal aspects of kinship and political organization, as a means of conceptualizing this total social structure. Hence the emphasis on the social groups described as permanent, on paradigms of lineage structure and other models of kinship. The family was in eclipse as an impermanent and untidy group which only existed because men of the agnatic lineage had to find mates outside their own ranks, and hence regrettably acquired in-laws, who might also be members of corporate lineages. The study of family attitudes dropped out of fashion and the interest in kinship terminologies declined. The emphasis on problems of cohesion and particularly on the question of the cohesion in segmentary societies was paramount.

Gluckman returns to the study of family attitudes, but he views them from the angle of agnatic descent groups to which he believes them to be a danger. Family affections must be pruned and kept in check in the interests of wider co-operation. The concept of conflict provides a way out. The paradigm of balanced segments is a conceptual model; in life there are clashing interests and quarrels. The hypothesis of the necessity and value of conflicts leads us back again to the model of the balanced society—that chimera which continues to haunt British anthropology.

A. I. RICHARDS

**Insight: A Study of Human Understanding.** By Bernard J. F. Lonergan, S. J. London (Longmans Green), 1957. Pp. xxx, 785. Price £3 3s.

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The wide variety of topics dealt with in this large work is not easy to describe briefly and its title is not very informative. Less than half the work is concerned with insight as an activity of the understanding. The remainder deals with the general character of the realities which this activity reveals, the assumption being that if we could understand the nature of insight we would know the main lines of all that there is to be understood.

From some passages it would seem that the author's purpose is to restate the more fundamental contentions of scholastic philosophy in terms of modern knowledge, freed from compromising associations with Aristotle's *Physics* and from dependence on an appeal to authority or self-evident principles (pp. 521-3). But the form which the work actually takes is an attempt to show the kind of world view or metaphysical system which a grasp of the nature of insight implies. In the early chapters Professor Lonergan examines and illustrates the nature of insight as it operates in mathematics and physics where we have its most precise and accurate expression and in common-sense judgements where we have its most concrete and practical form, but where the disinterested desire to know is apt to be distorted or restricted by various influences. He then proceeds to explain the method, the elements and part of the outline of the kind of metaphysical system which seems to him to follow from the nature of insight as he found it in science and common sense. 'The results of applying this method,' he finds, 'bear an astounding similarity to the doctrines of the Aristotelian and Thomist tradition' (p. 521). This is not so surprising because one suspects that the



procedure is guided throughout by a broad knowledge of the end to which it seems to the author to lead.

However able and knowledgeable Professor Lonergan may be (and he is both), it is difficult for him to maintain the interest of his readers for nearly 800 pages of highly abstract discussion, with few concrete illustrations and abounding in wide, necessarily unjustified, and sometimes dubious generalizations about diverse subjects ranging from mathematics and atomic physics to sociology and depth psychology and from the analysis of common-sense judgements to theories of history and the development of philosophy. The treatment in some sections is interesting and illuminating as, e.g., in the analysis of the change from mechanism to relativity and from determinism to statistical laws in recent physics, but other sections seem to be little more than putting commonplaces of scientific procedure into a semi-technical terminology in which they lose rather than gain in clearness. It is, however, the relation between the different sections of the argument and the resulting world picture as a whole which seems least clear and convincing.

No doubt the work is written, as he puts it, 'from a moving view point,' and we should not expect complete coherence between all its contentions. For what is said from earlier and lower view points has to be 'reoriented and reinterpreted' in the light of what is said from later and higher. (Incidentally, we are given no indication of what this re-interpretation involves but it would seem that the insights gained at the lower view points remain unchanged). These different view points are represented by different sciences natural and human, from mathematics and physics at the one end to metaphysics and theology at the other. These sciences are all said to be autonomous, but in view of the need for reinterpreting their results such autonomy would seem to be only relative. The order in which topics are dealt with or in which the view points are arranged in this work is not determined by logical or metaphysical priority but by what is likely to be most helpful to the reader in understanding the nature of insight and acquiring self-knowledge. And it should be added that men can not only acquire direct insight into what is systematic and intelligible but also inverse insight that certain topics are non-systematic and unintelligible, certain questions unanswerable—at least from this standpoint and with the categories of a particular science. When all that is intelligible is understood, there is still left an 'empirical residue,' something given or experienced which has just to be accepted, or conjunctions which seem incidental, not necessary or even highly probable. But what may be an incidental conjunction from one point of view, e.g. that of physics, may be intelligible from another, e.g. that of biology. It is these unintelligible residues, these unanswerable questions at each level that drive the enquiring mind from view point to view point till it reaches the highest view point of all, that of God or theology where 'the non-systematic vanishes' (p. 649).

The discussion throughout is not only abstract but very general, so general as to be often vague. As a result some of the parallels which are drawn in general terms between what we find in different sciences, and some of the connexions which seem plausible when stated in very general terms, will not bear close examination. 'Generalities,' as Professor Lonergan himself tells us, 'can be very misleading' (p. 726) and it is not much comfort to the reader who has toiled through hundreds of pages of generalities to be told that 'a correct philosophy . . . precisely because it is correct will be too complicated to be commonly accessible and too alien to sinful men to be

widely accepted' (p. 693), the implication apparently being that when he finds the going hard he should accept the view of the expert who knows.  
A. MACBEATH

**The Unconscious Motives of War.** By *Alix Strachey*. London (Allen & Unwin), 1957. Pp. 283. Price £1 5s.

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Social anthropologists will reject this book, not because it is psycho-analytic, but because it seeks to explain the existence of social institutions by a process of psychological reductionism, and a somewhat fantastic one too.

Two-thirds of the book are devoted to a necessarily dogmatic and selective summary of psycho-analytic theories (Kleinian type), which other psycho-analysts will accept or reject according to their persuasions. Anthropologically, there are two main, long-outmoded fallacies: that the pre-conscious portion of the 'savage' mind is far less developed than is that of the 'civilized' mind (p. 68), and that the members of a nation-state derive their unity in large part from belonging to the same race and thus sharing innate and distinctive psychological characteristics (p. 201).

For Mrs. Strachey, war results from the sum of the pre-genital aggressive drives of individuals, which are enhanced through a combination of fixation and regression. She argues that (apparently all) organized groups, despite their constructive functions, encourage regression in individuals and thus enhance their aggressive drives and their potentialities for war. 'A person's social environment . . . causes him, in other words, to have something akin to a mental illness.' Groups are, therefore, more or less bad for mental health, but some groups are worse than others, and of these, the State is the worst. It would be advantageous if states could be abolished, or weakened at the expense of other less dangerous groups, or combined into a World State without rivals, but the author does not think that any of these measures are feasible.

Alternatively, it might be thought that extremely permissive child-training could reduce both the aggressive drives and the 'group-proneness' of individuals. But here again there are insurmountable difficulties. For children must, for example, be frustrated to some degree if they are to be kept alive, and moreover, present-day parents cannot help hating their children to some extent and, however covertly, showing their hatred. Finally, even if it were possible, a wholly loving attitude on the part of the parent might arouse excessive guilt in the ambivalent child, or might be interpreted by him as sexual seduction and met with defensive hostility. Wars are not therefore to be abolished through reformations in the nursery.

Obviously then, the only solution would be for everybody to be psycho-analysed. But this too is not at present feasible. The populations of backward nations, for example, are too retarded mentally to profit from psycho-analysis, although this does not matter much because they do not yet have atom bombs. And of course there are not enough analysts. Finally, even psycho-analysis is not necessarily the perfect panacea, for destructive impulses, though modifiable, are believed by this author to be innate in men (and therefore, presumably, to some extent present in the psycho-analysts as well). Mrs. Strachey believes, however, that women are born with far less destructive and more libidinal energy than men, so that in the end ('who knows?'—p. 266) the emancipation of women may bring salvation to mankind. Well—who knows?

E. KATHLEEN GOUGH

**Dévotions et saints guérisseurs: Contributions au folklore charentais.** By *Marc Leproux*. Paris (P.U.F.), 1957. Pp. xxiv, 264, 8 plates, 15 maps

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The author has spared no effort to present a complete picture of the devotional practices in the Charente. Saints, farmers and pilgrims are vividly sketched; the indexes and maps are exemplary. The main regional features of the three *arrondissements*

under discussion are shortly indicated (p. xxii) and the numerous local shrines described in great detail. The puzzling question why the same saint may occasionally be regarded in one place as benevolent and in another as malevolent has been satisfactorily answered (p. 72).

The local prayers have been collected since the beginning of the nineteenth century. 'La Patenotre blanche' (pp. 15, 16) was also used

## EUROPE



for magical purposes. Mr. Leproux emphasizes how greatly the simple rhymes appeal to the uneducated. Very useful is the differentiation between the offensive and defensive aids to devotion: prayers and holy water serve both purposes; cross and candle are purely defensive (pp. xviii, xix, 50ff., 56ff.). Whereas the farmers resort to the same ritual for their sick relatives and cattle, they pay great attention to the right moment in order to direct divine protection on the harvest as well as the cattle (p. 52). Infinite care is taken by 'la récoumanderie,' a wise woman or white witch, to find out which saint has caused the disease. The variety of her divinatory rites is noteworthy (pp. 76-89).

Valuable are the descriptions of pilgrimages to menhirs and dol-

mens (pp. 62, 63) and of the various rites performed at large stones (pp. 64, 65). Stones bearing the marks of saints in the immediate neighbourhood of holy wells are too numerous to be accidental (p. 74). Just as in Wales and Ireland they must have been brought to the wells. A parallel to the Celtic belief in second sight (*cf. Folk-Lore*, Vol. LIX, p. 117) is provided by the legend of St. Mathurin (p. 168). Charlemagne and pilgrims to Compostella passing through the Charente are several times mentioned. During the Hundred Years War the English left unharmed those pilgrims who wore a badge of St. Eutrope (p. 192). For a second edition a number of misprints should be corrected and better photographs provided.

E. ETTLINGER

## OCEANIA

**The Papuas of Waropen.** By G. J. Held. *The Hague (Nijhoff)*, 1957. Pp. xv, 384, plates, text figs. Price 14 guilders

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This is a translation of a monograph by the late Professor Held (see MAN, 1955, 163) which was published in Dutch in 1947 and reviewed in MAN by Dr. E. R. Leach (1949, 92). It is, as Leach wrote, 'an extremely able piece of work' and its translation from a language which the great majority of anthropologists do not know is very welcome.

The present version is the second in the newly instituted *Translation Series* of the Koninklijk Instituut voor Taal-, Land- en Volkenkunde, and is produced in the style and with the thoroughness which we associate with that learned institution. The translation is in general accurate and clear, and apart from a few mistakes of idiom reads very well. Certain technical terms, however, are misrendered and prompt the suggestion that it would be useful if future translations were checked by scholars whose native language is English and who would also be able to propose correct versions of terms proper to their disciplines. In the present case it is incorrect, for example, to say of a patrilineal (*vaderzijdig*) lineage that it is 'paternal'; and to refer to a certain exchange relationship (*ruilrelatie*) as 'barter-relations' tends to obscure the importance of these prestations for a general theory of exchange. In general, also, there is no point in translating the titles of articles and journals, especially when *Adatrechtbundels* becomes rather incongruously 'Bundles of Adat Law.'

The monograph deserves special attention now because it deals with a type of society brought recently into prominence by the work of Lévi-Strauss and Leach. Waropen society is characterized by a matrilineal prescriptive marriage system, marriage with the 'mother's brother's daughter' being prescribed (*voorgescreven*, not merely preferred, as Leach has it). The kinship terms given accord with this form of marriage-regulation, with the startling exception that the term for the patrilineal cross-cousin (who cannot be married) is the same as that for the matrilineal cross-cousin. It is not possible to discern precisely how this system worked, though it is clear that Waropen symbolic classifications, economic exchanges and affinal status relations concurred with what we have learned to expect with this sort of prescriptive system. It seems that Waropen society was observed at a period of rapid change (they were all Christian, and traditional gift-exchanges had lapsed), and this may partly explain why the analysis does not meet our expectations; but some influence must also be ascribed to the stultifying *idée fixe*, still current in Dutch anthropology, that this type of society must be derived from an original eight-section system. It is regrettable that ethnography of this quality should have been misled by such a fruitless notion, but the work nevertheless amply deserves the place in every library and the wider public which the present translation ensures.

RODNEY NEEDHAM

**A Dictionary of the Nggela Language (Florida, British Solomon Islands).** By C. E. Fox. *Auckland (Auckland Inst.)*, 1955. Pp. 271

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Dr. Fox of the Melanesian Mission, after half a century as a priest and scholar in the Solomon Islands, has succeeded

in bringing his dictionary through the press. Works of this nature tend to be published only after a long series of unsuccessful efforts to secure the support of a Mæcenæ. Yet all too often the first and only edition is soon out of print, and second-hand copies may in the course of time even acquire a scarcity value. This dictionary is of special interest in view of Dr. Fox's long advocacy of Nggela as a *lingua franca* for the Eastern Solomons. Where linguistic communities are small and there is no history of a native hegemony, there is a call for a consistent policy on the part of government and missionary bodies if a language is to gain universal currency. In the Eastern Solomons (as opposed to the Western part of the Protectorate) a number of factors have militated against the general recognition of any one *lingua franca*. As a result each mission tends to resort to expedients and education suffers.

The work under review has the merit of listing numerically the different acceptations of a single word. In general a too rigid grammatical nomenclature is rightly avoided. It might seem a little ungracious to complain of an insufficiency of citations. Nevertheless when a citation is provided, it is not always related to the translation. Sometimes it is intended to make the meaning of a word clearer: thus *nonohi* (p. 127) and *ranggotagi* (s) (p. 173). Sometimes it is an illustration or elaboration, that is to say the Nggela words that follow the English translation bear no immediate relation to it but presumably it is something that a Nggela-speaker could say, or has actually said *à propos* of the head word. Thus (p. 168), '*pugugumba*: Filthy, of one who doesn't wash daily; *e uru na hulina*'; the last few words (for which no translation is given) correspond roughly to: 'his skin has a bad smell.' It is not clear, however, what the relationship of this Nggela sentence is to the head word. It cannot be doubted that the work would have gained if Dr. Fox had confined himself to actual citations, such as that given for *nggonggora* (p. 145), 'To drift, drag, of anchor; *te nggonggora na pinititi*,' which I presume means: 'the anchor is dragging.'

One of the main difficulties for a would-be comparatist in a preliterary field of philology is that without historical evidence he cannot posit a common cultural background and the width of the semantic scatter (even when dealing with relatively simple concepts) cannot always be ascertained. Thus Samoan *asu*, smoke, and Nggela *ahu*, smoke, give a provisional *s=h* correspondence. But Samoan *aso*, day, gives Nggela *aho*, sun, while S. *sina*, white, gives in Nggela *hina*, glow, shine. There is however one field of comparison where semantic variation may be relatively negligible, and that is the nomenclature of animal and vegetable species. Where the scientific term is known or accessible to the lexicographer (and a botanist or zoologist not infrequently does give native terms among other items of information) then there is a good case for giving it in a dictionary. Here for purposes of comparison it would for instance have been of interest to know whether *hinu* (p. 36) a tree, is the same tree as that called in Fijian *sinu* (*Leucosmia burnettiana*, Thymelacaceæ) and whether the ginger *ria* (p. 177) is the same as the Fijian *layalaya* (*Zingiber zerumet*, Zingiberaceæ).

All social-science workers in the Western Pacific area, and particularly the linguists in their midst, will welcome the appearance of this latest book by Dr. Fox and look forward to yet other works from his courageous and indefatigable pen.

G. B. MILNER









(a)



(b)



(c)



(d)

- (a) Stuffed raccoons in the roof trellis  
 (b) The drum (gong) under table B  
 (c) The three San Juans and the little Virgin  
 (d) A day-time deer and tiger dance in the compound of San Juan. The tiger holds a stuffed squirrel in his hands.

### COFRADIA SAN JUAN

Photographs: E. M. Mendelson. The bundle itself could not be photographed since no flash was available and all doors are closed day and night.



# A GUATEMALAN SACRED BUNDLE\*

by

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**170** The religious ritual of Santiago Atitlan, a Tzutuhil Maya village in the Solola Department of the Southern Guatemalan Highlands, takes place, almost exclusively, in its ten *cofradías*. These are small chapels within each of which the cult of one saint is celebrated by a body of religious officials ranked in hierarchical order with an *alcalde* as their leader. These officials are men climbing the politico-religious ladder of office by means of 'services' rendered to the village in one or more of the *cofradías* and in the Municipality. Broadly speaking, a man serves alternately, year by year, in each of the two hierarchies until, having rendered a certain number of services, he emerges as a *principal*, an adviser to the religious head of the village and to the mayor. In any given year, each *cofradía* is located in the house of its *alcalde* who is responsible to the head of the village for the performance of the saint's ritual in that year. The next year, the *cofradía* paraphernalia moves on to another *alcalde*'s house and a different set of *cofrades*. Public ritual is performed by the *cofrades* as representatives of the *principales* and the Municipality on the saint's day (*fiesta*) with prayers, drinking and feasting in the *cofradía* and processions of the saint's statue or statues to and from the church. Private ritual for specific individuals or families is conducted in the major *cofradías* all the year round by native priests (*ajkun*, i.e. prayer-makers, healers and diviners) who have no place within the public politico-religious hierarchies.

Although *cofradías* are often claimed to be equal in rank, the observer soon discovers that some of them are more important than others: ritual is celebrated more often and more abundantly, *ajkun* visit more frequently, extra officials stay with the *cofradía* on a permanent basis as it moves from *alcalde* to *alcalde* year after year. In the case of *cofradías* Santa Cruz and San Juan, this added importance is associated with the presence of two cult figures: the Maximón doll and the San Martín bundle respectively. These two cult figures differ in many ways from the ordinary wooden saint statue. I hope to show that they are in fact contemporary versions of ancient Maya divinities which have found a place for themselves as saints at the heart of the *cofradía* system. This article is devoted to *cofradía* San Juan and its sacred bundle the San Martín.<sup>1</sup>

*Cofradía* San Juan (see fig. 1), though at first sight similar to any other, contains various extra features of interest. The ceiling trellis is not only hung with various tropical leaves and fruit but also with some 12 or 13 stuffed raccoons (Plate Ja). On a shelf just below the ceiling (A) are disposed some 30 older stuffed animals, mostly raccoons, and odd

pieces of animal skins. A table (B) bears several complete deer skins, some with skulls and horns attached, and two or three jaguar skins (fig. 2). Under the table lies an ancient two-tongued wooden drum (C) featured only in *cofradías* of major ritual importance (Santa Cruz, San Antonio, Santiago and Concepción) (Plate Jb). The altar table bears three statues of San Juan (D, E, F), the largest (D) and smallest (F)

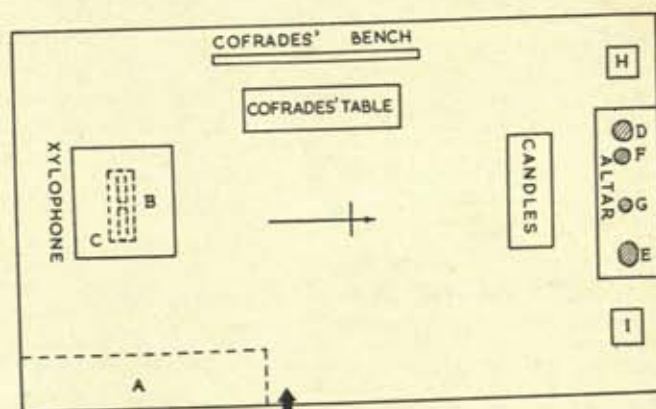


FIG. 1. PLAN OF COFRADIA SAN JUAN

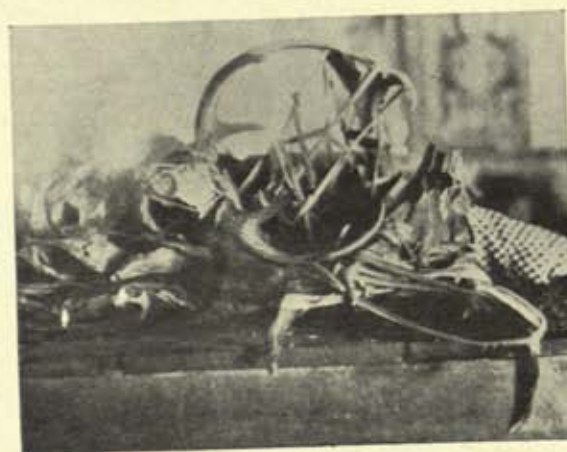


FIG. 2. HORNS OF DEER COSTUMES ON TABLE B

holding a Bible surmounted by a lamb. There is also a little Virgin in a painted box (G) (Plate Jc). Two wooden cases (H and I) placed one on each side of the altar are rarely opened. One of them contains the San Martín bundle (H). *Cofradía* San Juan celebrates two main *fiestas*, that of San Juan on 24 June and that of San Martín on 11 November.

\* With Plate J and two text figures



The latter appears to be the more important since it is also celebrated by *cofradías* San Antonio and San Nicolas, both of whose altars bear statues (not bundles) of San Martin on his horse. *Cofradías* San Juan and San Antonio are in many ways related: they share with *cofradía* Santiago the honour of extra details in the transfer of power ceremonies at the end of the year and both little San Juan (F) and his corresponding little San Antonio take the lead in all processions of saints from any *cofradía* to and from the church. In addition to this, a certain Dance of San Martin is performed on seven major *fiestas* in *cofradía* San Juan: San Juan, San Martin, Santiago, Holy Week, All Saints, Corpus Christi and (probably) New Year. Thus, while most *cofradías* limit themselves to one *fiesta*, San Juan is busy all the year round. San Martin is the only Atiteco cult figure present in more than one *cofradía*; he is also found in two private houses, called *cofradías* by extension, *cofradía* San Martin Particular (which has a second bundle) and one other which has a small statue. It remains to be said that *cofradía* San Juan has an extra, permanent official, the *nabeysil*. Like the dresser of the Maximon doll (*telinel*) in *cofradía* Santa Cruz, he is an *ajkun* and wields great power in the village as an intercessor. He is responsible for the Dance of San Martin and should remain unmarried and chaste for as long as he holds office. He should also have a special 'power' (*nuwal*) or 'feeling for the job' (*sentido*) without which 'he would not be strong enough to lift the bundle.'<sup>2</sup>

The Dance of San Martin is the most esoteric item of Atiteco ritual: no village Ladino, however old a resident or otherwise versed in Indian customs, seemed to know of it and most younger Indian informants had only the woolliest notions about it. I now describe it as I witnessed it during *fiesta* San Martin, 1952. On the evening of 10 November, the *cofradía* was full of *cofrades*, members of the *alcalde's* family and visiting *ajkun*. A *marimba* team played behind table B. On the altar table, in front of the statues, lay a rectangular bundle, some 24 by 12 inches, covered in red cloth, slit horizontally along the top. On the narrow slit lay five flat rectangular cakes of hardened corn meal. On the bundle's left lay a small apron of disintegrating cloth with little wooden, colonial-style angel faces sewn onto it.

At 11.30 p.m. the *nabeysil* gave the signal for proceedings to begin. Four young men seized the skins on table B. Two put on deer (*masat*) costumes, composed of a head-to-ankle-length back piece bearing the skull and horns decorated with a criss-cross pattern of green and red cotton ribbons and a square waist-to-knee apron front piece, tied with string to the shoulders of the back piece and held primly in both hands by the 'deer' while dancing. The two 'tigers' (*bajlam*) wore back pieces only and each carried a stuffed squirrel in his hand. The *marimba* played and the four dancers moved in circles, hopping from foot to foot and swaying from side to side, occasionally whirling round in one spot, the 'tigers' emitting long whistles and sharp cries and pawing the backs of the 'deer' with the squirrels. Four times the group knelt abruptly, one behind the other, and crossed themselves, thus saluting the four cardinal directions at three-to-four-minute intervals in the dance. They then went into the courtyard, performed again, returned, kissed

table B and lit a candle in front of the drum, took off the costumes and danced again, this time saluting eight directions.<sup>3</sup> All the while, one individual said to be the leading 'tiger' and 'very wise in the dance' swung an incense-burner over and around them. This man, with one assistant, now repeated the dance as the 'deer,' and, in the courtyard, a real battle was enacted, the 'deer' striking with his horns and the 'tiger' assistant with teeth and paws. Eventually the 'deer' died, climbed onto the 'tiger's' back and was carried into the *cofradía*. Costumes were taken off and the leader danced once again alone, more leisurely, with knees flexed and legs passing alternately in front of each other, arms held outstretched, palms held straight and facing inwards.

At midnight, the *nabeysil* ordered the doors and windows of the *cofradía* to be shut, approached the altar and knelt before the bundle. From under the corncakes he extracted a short beige shirt covered with designs resembling conventionalized tongues of flame. This he put on while lit candles were distributed among the now silent assistants. With much deliberation, he then danced in a similar fashion to the 'tiger' leader, motioning people out of the way, his eyes shut as if in a trance. After dancing to the four corners of the room, he stopped with his back to table B, leaning slightly against it, arms outstretched sideways, legs crossed at the shins, head lolling on the right shoulder, face (constantly wiped by attendant *cofrades*) anguished, as if crucified.<sup>4</sup> One by one, *cofrades* first, his public knelt before him, crossing themselves and kissing his belly, hands and feet. One man kissed the belly as altars are kissed: centre point, point to the left, point to the right. A *cofrade* began beating the drum and the *marimba*, silent during the kissing, now joined in. The assistance then kissed each altar saint while the *nabeysil* went back to the bundle, took off the shirt, crossed himself to the four directions, took out another shirt and repeated the whole performance.

I never witnessed the 'crucified' position again, though on other occasions I saw the same dance performed with the *nabeysil* carrying the corncakes or the whole bundle. Usually the dance is performed about three times per *fiesta*, as close to noon or midnight as faulty watches and general drunkenness allow. The deer and tiger dances usually precede it and these dancers, said by some to be *ajkun* in training, also precede the *cofrades* to church when they inform San Juan on his altar there that they have taken him in charge for a year (Plate Jd). On one of my last evenings in Atitlan, I saw the *nabeysil*, very drunk and reluctant to dance, drop the bundle. After a moment of dead silence all present rushed to the altar, knelt and prayed frantically. The *nabeysil* was then held up by the *alcalde* and the 'deer' until the dance was over. Whether the fact that the *nabeysil* had recently asked the *principales*—in vain—to relieve him of his 'burden' had anything to do with this episode or not remains problematical.

Despite the very confused state of Atiteco beliefs, some enquiry is now needed into the meaning of this Dance of San Martin. I must first recall the Indian belief in *dueños*, the supernatural owners of the various aspects of Nature, a belief which has blended quite satisfactorily, in the *cofradía* system, with the Roman Catholic belief in patron saints.



While San Antonio is *dueño* of domestic animals, San Juan looks after the wild ones which are 'of God': their skin can be sold and their flesh eaten, but their bones must 'go back to the hills whence they came.' Hence the raccoons and skins brought in by successful hunters in thanksgiving. The deeper one pries into Atiteco beliefs, the more *dueño*-ships are found to drift from the control of ordinary *cofradia* saints into the hands of San Martin until one finds him defined as head of all the *dueños* who act as subservient 'angels' at his royal command. In this context he is always called *Rey San Martin*, the King. It is believed that only at noon and midnight can the bundle be safely opened: otherwise all the winds would break out of it and 'wreak havoc in the world'; in any case, doors and windows are shut during openings and the bottom shirt in the bundle, being the most 'powerful,' is never brought out.<sup>5</sup> When the Martin emerges at these times 'he must walk about over the hills and volcanos and all the Departments to give his orders to his angels,' 'the houses of the angels being in the hills and valleys and clouds where they work and give the plants and the food and the rain.'<sup>6</sup> The dance, in the *nabeysil*'s words, is 'a kind of confession, not of sins, but for the asking of beans and corn since we Atitecos are poor people'; seed corn, indeed, is often blessed in the *cofradia* before the San Martin box. One *ajkun*, Baltazar, had evolved a systematic set of beliefs about San Martin in which cross-fertilization between old Maya and Catholic beliefs was very evident, particularly in the use of the numbers 12 and 13 derived both from Maya calendric day numbers and the 12 Apostles plus Christ. For him San Martin was the '*dueño* of the whole world, older than any other saint and father to them all; each village might have its Martin, but it is also true that never in all my travels have I seen a bundle like ours and therefore it is true that Atitlan is *remeshush jap*, the navel of the rain and *remeshush ulewu*, the navel of the earth, the centre of the world.'<sup>7</sup> He also held that among the hosts of heavenly beings there were 12 principal Martins, 12 Marias and 12 Angels. Though this seemed to have arisen from a spontaneous stylistic trick of *ajkun*'s prayers—citing a basic name, then making a long list by repeating it with secondary names attached—he did produce lists for me of this heterogeneous 'company of the holy world': a mixed crew of Catholic angels and archangels, saints, Maya calendric day names, wind names, ritual-object personifications (*lit.* god-candle, god-incense, etc.), personages from Spanish-Indian festival dances—kings, soldiers and devils—and (I shall come back to this later) certain human beings who turned out to be dead Atiteco *nabeysil*.<sup>8</sup> The Marias appear to be subordinated to the angel-faced cloth, kept in box I, which represents *Yashper* (Maria Ana or Maria Isabel), 'a very old woman of ancient times, crippled and bent but still powerful who opens the path for children' and is prayed to by the *iyom*, the midwives, female equivalents to the male *ajkun*. Some informants called the cloth a representation of the 'insides of a woman' (*las tripas*). Sick children are sometimes clothed in little red and green shirts contained in box I and cradled in the box for a while by *ajkun* called in by their family. The association of human, animal and vegetal fertility and wellbeing which is so

strong in all Maya ritual is thus consecrated in *cofradia* San Juan.<sup>9</sup>

During one dance of San Martin, some *cofrades*, usually impervious to such matters, pointed out to me that thunder could be heard and that rain could be expected. The true nature of this performance as a rain-making ritual appears nowhere more clearly than in accounts of the deeds of dead *nabeysil* said to have lived some 50 to 70 years before my visit. 'In those days, whenever the village needed rain, Santiago, San Juan and San Antonio were clothed in green "cloaks of rain." The bundle was brought out and various tricks were performed such as lifting the *cofradia* table into the air by going up to it and making as if to bite it. Then there were processions which brought on the rain. There was also another custom after the rain to ask for the sun back and at this point red cloaks were worn by the saints.' The colours of these capes suggest a meaning for the red and green antler ribbons of the deer dancers, the shirts in the *Yashper* box and the similarly coloured procession stretchers found all over this area of Guatemala. Today in Atitlan, a rain-making ceremony is performed by the main *ajkun* at the mid point of five official sowing-of-corn dates on *fiesta* San Felipe (5 February), but I found no trace of a sun ceremony. The respective positions of the San Juan and San Martin *fiestas* in the ritual calendar do suggest the possibility of some kind of equinoctial ritual having existed in the past, but more research is needed on this point.

Finally, there is a set of Atiteco legends about a family of ancestors (variously described as six to 12 brothers and six to 12 sisters, six married and six unmarried brothers, 12 brothers and their wives) whose exploits are situated 'in the beginning of the world.' These tales contain items manifestly as old as the *Popol Vuh* and the *Anales de Solola* mixed with references to biblical lore and to wars between Atitlan and Antigua which might have taken place just before the Conquest or in fairly early colonial times.<sup>10</sup> One informant gave a list of ancestors, some of whom reappeared in the *ajkun*'s list of *dueños*: many of the surnames were identical, thus confirming some sort of relationship between some or all of the ancestors. At one point in these tales, the brothers are locked up in an Antigua prison after killing an enormous Negro with the aid of a double-headed hawk, the *klavikoj*, familiar spirit to San Martin. Their sisters or wives make and bring to them the shirts of San Martin. Putting them on, the brothers arouse a great storm which destroys Antigua and sets them free, an event witnessed by all the tigers in the land gathered on a mountain top, i.e., presumably, the familiar spirits of *ajkun* and magicians. I will elaborate one interesting fact below, namely that a similar adventure befell the most famous of the more recent *ajkun*, Francisco Sojuel, and led to the creation of the second bundle in *cofradia* San Martin Particular.

A few remarks of a historical nature, however inconclusive, remain to be made. The shirts seem to me to require some explanation beyond the Atiteco version of their origin. The flame design on them resembles so closely the pattern on the conventionalized fleece (actually camel hair,



but to the Indian fleece) worn by statues of St. John the Baptist that we may take them to be copies of the fleece. Perhaps the Indians, upon hearing early sermons on the Baptist living in the desert and preceding the Christ, at some moment associated Juan and Martin with the old gods and the undomesticated aspects of Nature. Why, on the other hand, should the *nabeyzil* wear the shirts? Various items of the San Martin cult taken together suggest to me that this might be connected with the ancient custom of wearing sacrificed human skins by priests of Xipe Totec, the flayed god. There are references in the literature to dances not unlike the deer and tiger dance—though featuring other animals as well—taking place as late as 1620 around Mazatenango, a town closely linked by trade with Atitlan and also, I suspect, by religion: many of the ‘angels’ of San Martin are patrons of villages in the Mazatenango areas. J. E. S. Thompson feels that these dances are ‘almost certainly’ associated with Xipe Totec.<sup>11</sup> Further back still, the *Popol Vuh* gives the deerskin as a symbol of a major god Tohil, associated with rain, thunder and human sacrifice. Tohil was the god of Balam-Quitze, first of the Quiche ancestors (of which there were four, the last, in some accounts, unmarried), who, upon dying, left to his sons a ‘bundle of majesty’ as a ‘symbol of his being’: ‘“This is a remembrance which I leave for you. This shall be your power. I take my leave filled with sorrow,” he added. Then he left the symbol of his being, the *Pizom-Gagal*, as it was called, whose form was invisible because it was wrapped up and could not be unwrapped; the seam did not show because it was not seen when they wrapped it up.’ In a note on the bundle, A. Recinos refers to other such among neighbouring Maya tribes as well as to Torquemada’s mention of a Mexican Indian bundle ‘made of the mantles of the dead gods.’<sup>12</sup> Now it is clear from precolumbian data that some relation was thought to exist in ancient Maya ritual between a god, his sacrificial victim (human, or animal substitute) and the priest who impersonated the god, especially when wearing the victim’s skin. Clearly too, we have an equation in modern Atitlan between the dead *nabeyzil* and the gods in the form of San Martin angels. Can we also claim to have an equation between the living *nabeyzil* and a victim whose sole surviving symbols would be the murdered ‘deer’ of the deer and tiger dance and the shirt—or skin—of the Dance of San Martin? Though at no stage was the *nabeyzil* ever said to be San Martin, the ritual taken together with the belief in the great *dueño*’s emergence at noon and midnight certainly suggests an impersonation on his part.

In this connexion, a note by Tozzer, quoting Roys, is of great interest: ‘Roys points out that Crucifixion was associated with the worship of the rain gods and the cenote cult and that . . . one of the first missionaries reported that the Cross was adored as a god of water and rain.’<sup>13</sup> Early nativistic movements among the Maya featured crucifixions of children and adults as part of the rain ceremonies, a fact which may afford a clue to the origin of the ‘crucified’ position in the San Martin Dance though one other observer, Dr. Borhegyi, suggested a search for Franciscan influences here: the notions of crucifixion and sacrifice are

not, in any case, far apart.<sup>14</sup> Finally, though I obtained nothing on a possible relation between the *nabeyzil*’s job and his shrivelled leg and arm during my stay, it is interesting to find in Sahagun the following comments on disease and the rain gods (Tlalocs): ‘The various diseases for which they made promises to the Tlalocs were the gout . . . also contractions of tendons in any part of the body . . . or contractions of any member, limbs or arms, or for paralysis. . . . They also said that if anyone suffered from a shrivelled hand or foot . . . all this happened to him because the Tlalocs were angry with him.’<sup>15</sup> Could there have been, in modern Atitlan, a survival of the idea that the rain priest’s assumption of his office coincides with some kind of expiation or ‘confession’ of sin?

Comparative research in other Tzutuhil villages and among the neighbouring Quiche and Cakchiquel would probably yield further material on these difficult points. Ruth Bunzel is worth quoting on Chichicastenango: ‘The vegetative aspect of the earth is worshipped under the name of Diego Martin, a name arbitrarily chosen when the first missionaries forbade the use of the names of the ancient gods. By verbal analogy he is identified with San Martin, who thereby has become *dueño* of the earth, and his day (11 November) is observed with ceremonies at mountain shrines. . . . The other saint who figures prominently in agricultural ritual is Santiago. Here, as in Spain, he is the patron of horses, who tramples on the corn. As destroyer of the *milpa* he is vaguely identified with Jurakan, the god of the tempest, who has been baptized under the name of Manuel Lorenzo.’ In view of persistent rumours of rivalry between Atitlan and Chichicastenango, it is interesting to note that the destructive aspect of the wind is associated with the patron of Atitlan. San Juan, in Chichicastenango, ‘is identified with the forces of destiny that rule men’s lives.’<sup>16</sup> He is apparently the giver of the familiar spirit and of the ‘*suerte*’ or fate of each individual, and each child must be presented to him at birth. Similar ideas exist in Atitlan, some of them, as I have shown, associated with the Juan-Martin-Yashper ritual complex.

In conclusion, a word should be said about the place of the San Martin cult in Atiteco religion as a whole. However many items of Catholic belief may by now have entered into the cult, it does remain that part of the whole which most closely corresponds to what we know of ancient Maya religious life. The fact, already mentioned, that the most famous of the relatively recent *nabeyzil* created *cofradia* San Martin Particular after borrowing a shirt from the main bundle when imprisoned by some enemies of his—the shirt helped him through its rain and sun power to escape from a deluge and a fire—, as well as the general similarity of behaviour and destiny ascribed to all *nabeyzil*, prompts me to suggest a hypothesis regarding their role in Atiteco history.

Some beliefs as old as any obtainable in the Maya area have both reacted upon and been influenced by the deeds of certain native priests of the turn of the century. The older the informant, the more clearly he sees an equation between the deeds of *dueños*, ancestors and a long line of *nabeyzil* who have come to the rescue in Atitlan’s time of



need, which leaves a very thin dividing line between the human and the divine in one sector of Atiteco world view. Today unusual or eccentric individuals are still granted miraculous powers and the Indians are constantly on the look-out for such characters, despite many younger people's assurances to the contrary. Even I, after participating in certain rituals, was ominously referred to as the son of Francisco Sojuel. I suggest that the San Martin beliefs, if we may—however arbitrarily—isolate them for a moment from the others, represent a survival of a cyclical view of history in which both problems and those who solve them recur in a similar fashion time after time. Similar but not identical since circumstances are bound to change, these problems always involve a mechanism of salvation based, in the last resort, on the original model of the rain priest's salvation of natural abundance through his own special 'power' or 'destiny.' I have tried to show elsewhere that this Maya or 'native' view of history can conflict with other views, associated with Ibero-Catholic culture, in which history is seen as a straight line involving a succession of discrete events. This conflict may express itself in terms of a third view, which shares in the first in that it is repetitive, but also in the second in that repetition is here overwhelmed by historical circumstance, by a 'death of the world' which has robbed modern men of their magic and whose origin I have suggested locating in the Conquest's traumatic introduction to 400 years of acculturation.<sup>17</sup> In another place,<sup>18</sup> I have tried to tie these three views to three cults: that of the 'native' San Martin, that of the Catholic Jesucristo and that of the dualistic Maximon. I have also wondered whether the fundamental Atiteco crisis in the coming together of the two religions did not occur when a theory of sin brought about by disordered sexual relations and expiated by the consequent fertilization of Nature clashed with another theory of sin similarly incurred but only expiable through a divine moral law at odds with any heathen theory of salvation through fertility.

## Notes

<sup>1</sup> This material is extracted from 'Religion and World View in Santiago Atitlan,' Microfilm Collection of Mss. on American Indian Cultural Anthropology, No. 52, University of Chicago Library, 1957 (here called Long Text, L.T.), and a shorter Ph.D. thesis of the same name in the same Library. In 'Les Mayas des hautes terres,' *Critique*, No. 115, Paris, 1956, I have offered some reasons for the peculiar position of Atiteco 'native' deities within the *cofradías*; to the north of Atitlan, their worship usually takes place in the hills outside the village. See L.T., pp. 159-61.

I hope to devote a separate article to the Maximon doll.

<sup>2</sup> No informant could give the etymology of this word. It might derive from *nabeij*, path, or a Tzutuhil equivalent (?) of the Quiché-Cakchiquel month lists word: *nabei*, great. J. E. S. Thompson, *Ethnology of the Mayas of Southern and Central British Honduras*, Chicago, 1930, p. 73, has a child-curing prayer in which the word *yabchil*, derived from the Spanish *llave*, means the 'key of.' See L.T., p. 310.

<sup>3</sup> No consistent directional pattern of saluting was observed. On this matter, see the ingenious discussion by D. E. Thompson in *Maya Paganism and Christianity*, Tulane, 1954, p. 13.

<sup>4</sup> Only one informant associated this with Christ's death and stressed the fact that the right leg should be crossed over the left. He belonged to *cofradia* San Martin Particular, whose dancer observed this pattern. The San Juan *nabeysil* was inconsistent.

<sup>5</sup> I give one version of an Atiteco story on the origin of thunder and lightning: 'According to the ancients, the angels have the right to visit women or wives every fifteen days. There was one angel who disobeyed this order and was tempted by a woman into seeing her in the intervals. One day after committing such a sin, he saw in a field the tree of the fruit *kushin*, took off his angelic clothes, lay down his angelic arms and climbed the tree. While he was eating there came in the air a huge snake . . . which curled itself around him and started sucking blood with its tail. The angel screamed for help. A merchant from Chicacao . . . was told by the angel to put on the clothes, take the arms and shoot the snake. The merchant was foolish, but finally did this and out of the weapon came a great lightning and the snake fell to pieces but the angel also died and turned black. At this point, the merchant was lifted into a great cloud, and this, since he did not know how to drive it, went at great speed towards the sea, with terrific rain which lifted houses and changed the course of rivers. The king of the angels, alerted, called his angels from their resting places in the hills and told them to catch the cloud before it fell into the sea, otherwise the world would be destroyed. They finally caught cloud and merchant and brought them to the king. The king told the merchant he would beat him, but the merchant accused the angel. Whereupon the angel who was alive again was beaten too.' Cf. L.T., p. 476, for analysis of Biblical and Maya overtones, also *The King, the Traitor and the Cross* (note 18 below).

One informant told me that thunder was caused by angels agitating their ornaments in the sky. On the shirts, see L.T., p. 516.

<sup>6</sup> Note here that Martin is the patron of Cerro de Oro (Hill of Gold), a dependency of Atitlan's in which much treasure is said to be hidden and the hill *dueños* are thought to rule and hold *fiestas*. There is a belief among the old in 'about three or four or six huge volcanos, situated in some other state or part of the world which, at noon or midnight, become the resting place for the throne of heaven.' Are these the four Bacabs of the old religion which 'stood at the cardinal points to hold up the sky'? (D. E. Thompson, *op. cit.*, p. 8).

<sup>7</sup> Cf. D. G. Brinton, *Nagualism*, Philadelphia, 1894, p. 46, who refers to a Quiche god *U-q'ux Uleuh*, whom he assimilates with the Aztec cave god *Oztoteotl* and the god of the heart of the mountain *Tepeyollotl*. On p. 50 he writes: 'Tepeyollotl, the Cave God, was patron of the third day and also "Lord of the Animals," the transformation into which was the test of nagualistic power.' Can the closed-door policy of the *nabeysil* ever have been related to cave ritual?

In view of the link between Maximon (Mam) and Martin, to be discussed in a forthcoming paper (Maximon and Martin are both *bundles* usually kept hidden), see J. E. S. Thompson: *Maya Hieroglyphic Writing*, Washington, 1950, p. 133f., for links between Mam and other gods of the centre of the earth and fire: *Tepeyollotl*, *Xiuhcuetl*, etc. Given Maximon's *dueño*-ship of sexual affairs, it is interesting to find Brinton, p. 54, giving *Huehuetotl* as the oldest of gods and governor of sexual relations, another of whose names is *Xiuhcuetl*.

<sup>8</sup> For a century-old reference to one such Martin, *dueño* of wind and hills in Ixtahuacan, north of Atitlan, see K. Scherzer, Vienna, 1856, reprinted as 'Los Indios de Sta. Catarina Istlavacan' (translated by E. Schaeffer), *Antropología e Historia de Guatemala*, Vol. VI, Part 2 (1954), p. 19.

For complete lists and interpretations see L.T., pp. 452-62, 471-8.

<sup>9</sup> In Holy Week, some 'races' take place between bearers of statues of Santa Maria and a San Juan 'Carajo.' S. K. Lothrop, 'Further Note on Indian Ceremonies in Guatemala,' *Indian Notes*, Vol. VI, Part 1, Heye Foundation, New York, 1929, refers to a custom, remembered but not carried out in my time, of imprisoning the statues to prevent the repetition of an affair indulged in by these two on the night of the Crucifixion. D. E. Thompson, *op. cit.*, p. 7, refers to the Maya association of Maria with the Moon goddess, patroness of childbirth and weaving, whose infidelity to the sun in mythical times had led to her attribute of licentiousness. In my time, an *ajkun* indicated that Sky-San Jose had created the world by copulating with Earth—Santa Maria.

R. Bunzel, *Chichicastenango*, New York, 1952, p. 111, refers to



'two married saints,' 'called by the name Sta. Esper, guardians of marriage and the domestic arts, especially weaving. They are two female figures who come out only once a year, on Good Friday.' If a wife refuses sex relations, these saints are prayed to (p. 118). On p. 271, a Santa Ana receives candles from midwives.

On the simultaneous fear of and desire for fertility in Atitlan, see L.T., pp. 376-84, and my concluding remarks on sin in this article.

<sup>10</sup> See L.T., pp. 514f.

<sup>11</sup> J. E. S. Thompson, *The Rise and Fall of Maya Civilization*, Norman, Oklahoma, 1954, p. 257, and E. Chinchilla, 'La Danza del Tum-Teleche o Loj-Tum,' *Antropologia e Historia de Guatemala*, Vol. III, Part 2 (1951). Thomas Gage (Broadway Travellers edition, London, 1928, p. 269) writes of dances very similar: 'a kind of hunting out of some wild beast (which formerly in time of heathenism was to be sacrificed to their gods) to be offered up unto the saint,' and O. La Farge and D. Byers, *The Year Bearers' People*, Tulane, 1931, p. 105, send us back to Gage in describing a kindred dance 'which came originally from Mazatenango.'

<sup>12</sup> *Popol Vuh*, English version by D. Goetz and S. G. Morley from the translation of A. Recinos, Norman, Oklahoma, 1950, pp. 175-192 and 205.

<sup>13</sup> A. M. Tozzer, *Landa's Relacion de las Cosas de Yucatan*, Harvard, 1941, p. 116.

<sup>14</sup> For the history of Maya nativistic movements, see D. E. Thompson, *op. cit.*, pp. 11-22. I have no doubt that some of the Atitico episodes about dead *nabeysil* refer to some such movements

and stress the importance of further research into a subject somewhat bedevilled by Brinton's romantic views on 'nagualists.'

J. E. S. Thompson (personal communication, 20 February, 1958) offers an interpretation which I had overlooked: 'I cannot help suspecting that the matter of the shirts may have nothing to do with any *Xipe* rites but may be a mixed-up dramatization of San Martin's offer of half his cloak to a beggar. The *nabeysil*'s shirt would then represent half the cloak, either as worn by the beggar or by the saint. Perhaps an incident in the saint's life would account for the crucifixion scene. A connexion between Juan and Martin might lie in the fact that St. John told his hearers to give away one cloak if they had two. The association of San Martin with rain probably arises from his being on horseback, as the *Chacs*, rain deities of Yucatan, are now horsemen, and the horse Cortes left at Tayasal was deified as a *Chac*. Mr. Thompson also stresses that the Mazatenango dance data allow no comparison with Atitlan's deer dance: the Mazatenango people said theirs represented sacrifice of a prisoner of war tied to a stake and attacked by jaguars and eagles as in the *Xipe* rites.

My own feeling is that the association of rain-making and sacrifice overrides this interpretation which could, however, have been a mask for the continuation of pagan rites.

<sup>15</sup> Bandelier's edition, Nashville, 1932, pp. 45-7.

<sup>16</sup> R. Bunzel, *op. cit.*, pp. 57 and 268.

<sup>17</sup> L.T., pp. 494-506 and 513.

<sup>18</sup> 'The King, the Traitor and the Cross,' *Diogenes* 21, Chicago, 1958 (also Paris, 1958, and Buenos Aires, Cologne, Rome, Cairo, forthcoming).

## A 'PELVIMETER' FOR ORIENTATION AND MEASUREMENTS OF THE INNOMINATE BONE\*

by

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**171** A conspicuous feature of the innominate bone is the twist or torsion around its long axis which results in the plane of the iliac bone being very different from that of the lateral wall of the true pelvis, the 'ischio-pubic' plane. The angle of 'pelvic torsion' can be defined as the angle between the iliac and ischio-pubic planes relative to the axis formed by a straight line joining the mid point between the 'anterior superior spine' and the 'posterior superior spine' on the iliac crest to the mid point between the 'symphysis' and 'ischial point' on the ischio-pubic ramus. In order to obtain a measure of torsion, it is necessary to orient the bone in a standard way with respect to this axis. Previous techniques for orientation (e.g. L. S. B. Leakey, *The Stone Age Races of Kenya*, London (O.U.P.), 1935; R. A. Dart, 'Innominate Fragments of *Australopithecus Prometheus*, *Amer. J. Phys. Anthropol.*, N.S., Vol. VII (1949), p. 301) do not permit of this being done, nor do they standardize the position of the bone from the point of view of linear measurement. A new instrument has therefore been designed and constructed, which allows of the standardizing of measurements regardless of major differences in the form of the innominate bone of different species.

\* With three text figures

The principal features of the 'Pelvimeter' are shown in figs. 1 and 2.

Within an outer ring *A* is an inner ring *B* which is graduated in degrees. The inner ring rotates in a groove in the outer ring, its movement being controlled by a knob *S*. An indicator *Ia* is fixed on to the outer ring so as to mark the reading on the inner ring. Fixed to the inner ring are two adjustable metal rods, with pointed tips *R1* and *R2*. The blunt end of rod *R1* carries a metallic protractor *P* which moves along an indicator *Ib* which is attached to the clamp locking *R1* as is shown in figs. 1 and 2. The whole apparatus is mounted on a square wooden base.

The innominate bone is held in position within the rings by applying the pointed tip of one rod to point *M* midway between the 'anterior superior spine' and the 'posterior superior spine' on the iliac crest, and the pointed tip of the other rod to point *M1* midway between the 'symphysis' and the 'ischial point' on the ischio-pubic ramus (see figs. 2 and 3). The line between these two bony points defines both the axis of orientation of the bone for purposes of linear measurement, and also the long axis on which it is twisted into its main planes. The bone is held in position by tightening the locking screws (see figs. 1 and 2).

In order to measure the angle between the iliac and



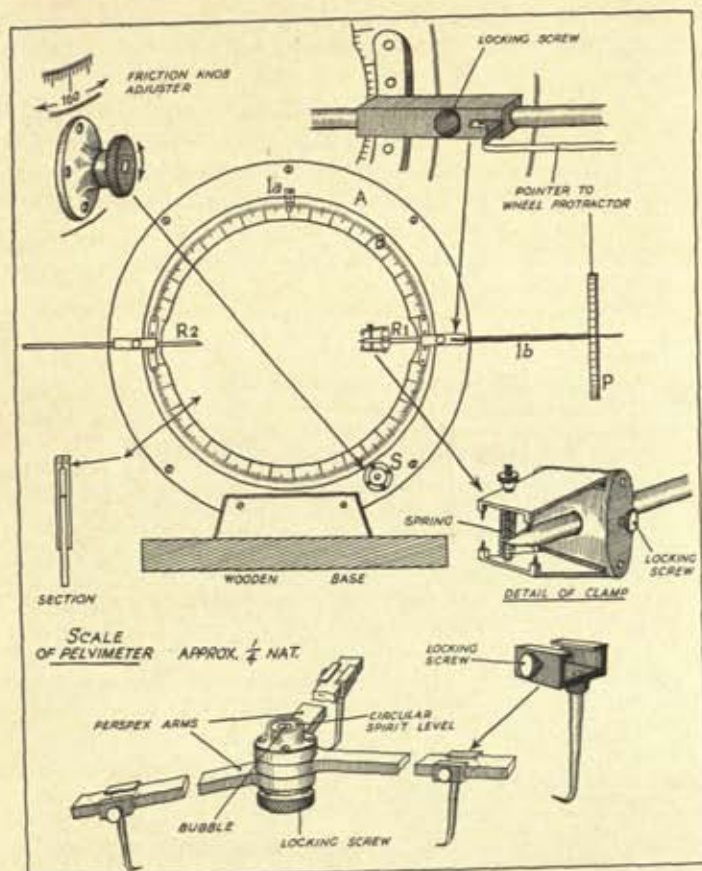


FIG. 1. DETAILED PLAN OF THE PELVIMETER AND ITS ACCESSORIES

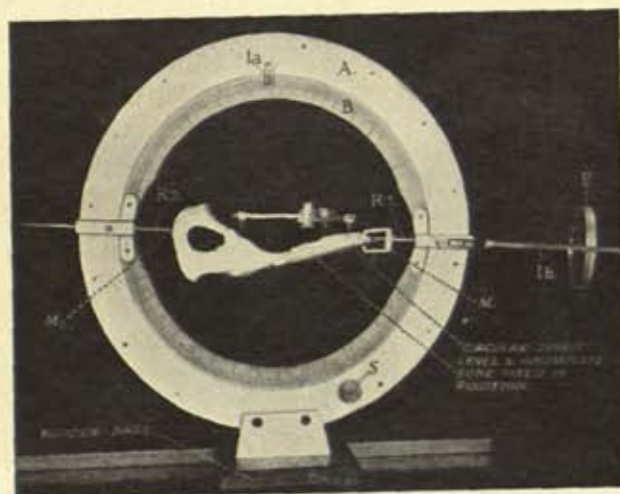


FIG. 2. PHOTOGRAPH OF THE PELVIMETER WITH A RIGHT GORILLA INNOMINATE BONE IN POSITION

ischio-pubic planes, each plane has first to be defined by three non-collinear points.

Unfortunately, however, neither plane is a flat surface. Apart from the 'angle of torsion,' the two are slightly bent towards each other, and are also characterized by minor irregularities. In the circumstances, the two sets of non-collinear points specified below are believed to provide the best

constant definition of the iliac and the ischio-pubic planes (see fig. 3):

#### I. Iliac plane

1. Anterior superior spine
2. Posterior superior spine
3. Iliopectineal point, which is taken as the deepest point on the iliopectineal line below the line defining the auriculo-pubic width, i.e. from 'symphysis' to the 'auricular' point (where the iliopectineal line meets the auricular surface).

#### II. Ischio-pubic plane

1. Symphysis
2. Ischial point
3. Iliopectineal point

By rotating the bone along its fixed longitudinal axis, using the protractor *P* as a handle, and by rotating the inner ring *B*, using knob *S*, either the iliac or the ischio-pubic plane can be made to lie horizontally. This is done with the help of a circular spirit level which is mounted on three perspex rods carrying three adjustable pointed limbs (see fig. 2), the first of which, when measuring the iliac plane, is placed on the 'anterior superior spine,' the second, on the 'posterior superior spine,' and the third on the 'iliopectineal' point. The bubble when central in the spirit level indicates that the plane is horizontal. The readings on the protractor *P* and on the scale on ring *B* are then noted.

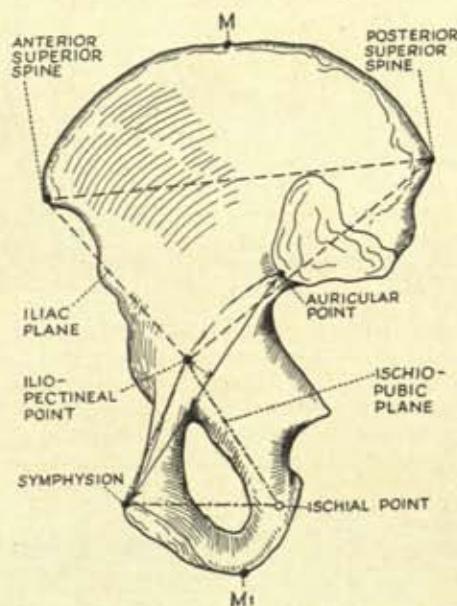


FIG. 3. A RIGHT HUMAN INNOMINATE BONE SHOWING THE POINTS THAT DEFINE THE 'ILIAC' AND THE 'ISCHIO-PUBIC' PLANES AND THE AXIS M—M<sub>1</sub>

In the second stage of the operation the bone is rotated along its fixed axis until the ischio-pubic plane is brought into a position where the 'symphysis,' the 'ischial' point and the 'iliopectineal' point also lie horizontally, as indicated by the spirit level. The difference between the two readings on the scale on ring *B* gives a measure of the angle of bend and the difference of the two readings on the protractor *P* the angle of torsion between the two planes relative to the main horizontal axis of the bone.



*Tests of technique*

In order to test the technical errors involved in the technique a sample of five chimpanzee innominate bones was selected. The angles of 'pelvic torsion' and 'pelvic bend' were then measured on the pelvimeter by five different workers without reference to each other's observations. Analysis of the data showed that the differences between individual bones were significantly greater ( $P < 0.001$ ) than the differences between the observers. This indicates that the technical errors involved when using this

instrument are negligible when compared with the differences between individual bones and that the pelvimetric technique described here is reliable.

My best thanks are due to Professor Sir Solly Zuckerman, C.B., F.R.S., for his interest and to Mr. W. J. Pardoe to whom I owe the illustrations in this paper. A grant from the Wenner-Gren Foundation for Anthropological Research Inc., New York, a part of which covers the expenses for the manufacture of this pelvimeter, is gratefully acknowledged.

## SHORTER NOTE

**An Early Mound at Luisville, British Honduras.** By Dr. Wolfgang Haberland, *Museum für Völkerkunde, Hamburg*. With two text figures

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During a short visit to Belize in October, 1954, I had the opportunity to visit different archaeological sites in the colony. This was made possible by the generous assistance by H.M. Government and especially by that of Mr. A. Hamilton Anderson, Archaeological Commissioner of British Honduras, who accompanied me during the various trips and to whom I wish to express my sincerest thanks.

On 27 October, 1954, one of the last days of the visit, we went north together to the small town of Luisville, 85 miles north of the capital and some miles south of Corozal. Some archaeological objects had been reported recently from there by a local resident, Mr. Luis F. Ramirez. Since only a few excavations have been conducted in this general area (with the exception of those by the late Mr. T. W. F. Gann) both of us were especially interested in this new site. Arriving at Luisville we were able to gather the following facts:

While a certain area near the house was being levelled off for the planting of sugar cane, the tractor used for this operation revealed several archaeological objects. Among them were a number of *chultunes*, some of them containing pottery, of which some specimens, now in the collection of Mr. Ramirez, showed unusual and interesting features, like the dark red monochrome spouted vessels (fig. 1). Nearby, during an attempt to level a shallow mound, the tractor struck something which at first looked like a dump heap of river boulders. The owner, sensing something special, stopped tractor operations immediately and began ex-

cavating. The boulders turned out to be the filling of a pyramid of which neither size nor form could be determined, owing to heavy weathering. More important was that further digging into the core revealed a second pyramid underlying the first one. Because only a part was excavated at the time of our visit its exact size could not be measured. Nevertheless, some quite unusual features (at least for this area) could be noted. The most important is that it is a round structure, of a kind only reported quite insufficiently by Gann from this area. It was entirely built of river boulders and coated with a thick layer of greyish lime, at the top as well as at the sides, which slope at a rather steep angle. Fortunately the excavation struck exactly one staircase (not necessarily the only one), consisting of five steps and built in the same manner as the pyramid (fig. 2). It was built into the body of the pyramid itself in a manner unknown from other known structures of this area. All these were astonishing facts, and I therefore looked round for some means of age-determination; this I found in some sherds picked up from the fill between the two structures. When compared with specimens in the Museo Nacional de Guatemala they turned out, to our great surprise, to belong to the Mamom Phase, the oldest known period of the Lowland Maya area. No other painted sherds have been found here.

Because of the position of the sherds it is highly probable that



FIG. 1. DARK-RED-PAINTED SPOUTED VESSELS FROM THE CHULTUNES, LUISVILLE  
Ramirez Collection. Photographs: W. Haberland, 1954



FIG. 2. THE STAIRCASE OF THE ROUND PYRAMID AT LUISVILLE  
In the background are seen boulders which form the fill between the first and second pyramids; remains of the platform coating of the first pyramid are at upper left.



the round pyramid of Luisville belongs to the Mamom period too. So far as I know, no structure of this time has been found up to date so far north, nor were round structures previously known from the Mamom period. Unfortunately, shortly afterwards and well before anything by the way of scientific excavation could be

done, a hurricane destroyed the pyramid as well as the very interesting private collection of Mr. Ramirez. But though this valuable piece of Maya archaeology is gone, it is to be hoped that more structures of this kind will turn up around Luisville and that it will be possible to excavate some of them scientifically.

## CORRESPONDENCE

### Bronze Age Technology in Western Asia and Northern Europe. Cf. MAN, 1958, 13, 39, 64

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SIR,—Although I am a firm believer in the Do-It-Yourself school of archaeology, I remain unconvinced by the theories advanced by Mr. Underwood to show that the axes of Bronze Age Britain were cast by the *cire perdue* method.

Mr. Underwood's main thesis would seem to be that since the most efficient use to which the bronze moulds from Southern England could have been put would have been for the preparation of models, this must have been their purpose. From this point Mr. Underwood argues, as indeed he must, that they were preceded by stone and clay put to the same use. The following observations are intended to outline briefly some objections to this argument.

(1) The bronze moulds from Britain come only from lowland England. From this area no stone or clay moulds are known of the Bronze Age, and it must therefore be assumed that the use of these moulds was an idea imported from the Continent. Properly it is from this source that the validity of Mr. Underwood's theory of their development should be checked, but since the bronzesmiths of highland Britain and Ireland were demonstrably influenced by craftsmen from the Continent, the evidence from this area cannot be disregarded.

(2) The stone moulds from the British Isles, chiefly of the Middle Bronze Age, are remarkable in that so many are of soapstone. This choice of so inaccessible a rock is only explicable if it is admitted that the moulds made from it were used for direct casting of bronze, for which it is, incidentally, admirably suitable. For this casting of wax models almost any soft rock that is not too porous would have sufficed.

(3) The clay moulds from the British Isles have come on several occasions from thoroughly and completely excavated sites, for example Jarlshof, Lough Gur, Lough Gara. Despite the fact that these were workshops, only two-piece clay moulds were recovered: no trace of the single envelope used in the lost-wax process was ever found. Furthermore, in two such moulds the inner surface has been found to have been altered to a green mineral product (Bray and Whitepark Bay) and a spectrographic analysis of this product from a fragment of the latter showed it to contain largely salts of copper, tin and lead.

(4) It seems inescapable to conclude that in this area the general practice was to cast directly into stone or clay moulds. It seems unlikely, but not impossible, therefore, that the bronze moulds from lowland England were put to an entirely different purpose.

(5) A random sample of socketed axes from lowland England will show well marked casting seams or flashes on a very large proportion, some of which project as sharp ridges three millimetres clear of the body of the casting. It is quite irrational to suppose that these would not have been removed from the wax model had such axes been cast by the *cire perdue* method, especially when the hammering and abrasion, clearly visible on many axes, required to remove these flashes is taken into account.

(6) It is in the reconstruction of the core, gate and runnels that Mr. Underwood seems to have come across the greatest difficulty, for, as he has shown in his excellent study of the casting of the Benin bronzes, the logical type of mould would allow the forming in wax of the axe, runners and header as an integral piece in this instance around the core. The bronze moulds from lowland England simply will not allow this, and to overcome this snag Mr. Underwood has had to resort to a somewhat fussy arrangement of clay wedges around a cast core. In this case, since only two runners were allowed

for, three clay wedges were used, but in the case of the 'Welsh' axe the arrangement would become even more complex since four runners were required which would have demanded five such wedges. No core moulds have, of course, ever been found, and Mr. Underwood assumes that they were therefore made of clay and have not survived. But since it would have been necessary to produce as many cores as wax models, this has the ring of special pleading.

If, on the other hand, it is allowed that the bronze moulds were in fact used for direct casting, no such difficulties arise. A clay shape can be formed in the mould and allowed to project above. When dry enough to handle the core can be pared down, the gate hollowed out in the top projection and the runnels skewered through.

(7) Axes with waisted sockets, it must be pointed out, are a phenomenon of the very end of the Late Bronze Age in Britain, whilst those axes that are demonstrably early have plain wedge-shaped sockets. Short of inverting the entire Late Bronze Age sequence, axes with waisted cores cannot, therefore, be looked upon as prototypes.

(8) The comments in (5) about casting seams hold equally true for many other types of implements, for example palstaves. The supposed traces of runners and risers in the Stibbard palstaves have an evenness that would suggest rather a defect in the matrix of the mould in which they were cast than evidence of wax modelling. Every palstave mould recorded from Britain shows pouring to have been from the butt end, and the side-poured, hollow, wax-casting suggested by Mr. Underwood, although in principle possible, is technically unsound.

(9) One would like to know the grounds on which Mr. Underwood states that beeswax was 'in short supply': at least in Mesopotamia, where he estimates that a handful of men could have produced tens of thousands of arrowheads in a few weeks, there can have been no such shortage. It need hardly be pointed out that this statement, if it can be supported in regard to Bronze Age Britain, goes a long way to undermine belief in the use of the *cire perdue* process.

(10) Had the *cire perdue* method been employed to the extent suggested by Mr. Underwood one would expect to recover quite a number of identical objects. In fact, except in hoards, identical bronze implements are something of a rarity. This in turn would imply that even the bronze moulds were relatively short-lived.

(11) The bronze moulds of Late Bronze Age lowland England cannot in fact be usefully studied by tearing them out of their context. The smiths who used them had to be constantly on the move because of the widely dispersed population which they served. Each move presumably meant carrying some at least of the bronze stock, all of which had to be collected as scrap or imported. In the circumstances clay moulds would have fared badly. But the same population required that half the smith's output should be axes or palstaves, in which case there could be nothing more rational than to make the moulds for these objects in a more durable material, for, with one exception only, it is for these implements alone that bronze moulds were used. Seen in this light the bronze mould ceases to have any technological significance whatever, being little more than the answer to a local problem.

(12) Finally, it should be pointed out that the shaft-hole axehead from Ur in the British Museum has at some time in the not-too-distant past been 'skinned'—that is to say the worst of the heavy corrosion has been removed mechanically. In fact the so-called 'blade-guard' would seem to be little more than a mass of corrosion left *in situ* in order to preserve the form in outline of the cutting



edge of the axe. If this view is not accepted it would become necessary to believe in 'butt guards' since at least one such axe has an identical covering over its butt end.

H. W. M. HODGES

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#### Notes

The above letter has been shown to Mr. Leon Underwood, who replies as follows:

To credit Mr. Hodges, as he asks in his letter, with a 'firm belief' in the empirical method is also to expect from him discussion informed by experience. In this he disappoints, failing to justify a confidence bold enough to pronounce upon specified objects under discussion—apparently without troubling to examine them. How could the stubs of wax runners on the Stibbard palstaves be seen as identical in anything but position? In form, no two in the whole group are alike. Then, with a reference to 'corrosion,' he dismisses the edge-guard on the axehead from Ur. Surely only corroded judgment could come to such a conclusion after examining the piece (see fig. 1).

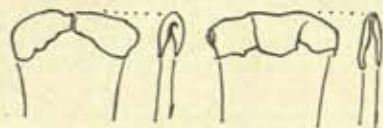


FIG. 1. EDGE-GUARD (?) ON THE AXE-HEAD FROM UR

*This sketch shows the reverse and obverse faces with one and two pleats due to wrapping of a metal plate over a bow-shaped blade; also top and bottom views*

(With regard to the Stibbard hoard, I regret that in MAN, 1958, 64, I gave the number of looped palstaves from a single mould as 17; according to the records of the British and Medieval Antiquities Department of the British Museum, the hoard includes two such groups of identical palstaves, eight of the looped ones, with wax runner stubs at the sides, and 16 without loops, the waxes for which were undoubtedly poured from the butt end.)

Further discussion of Mr. Hodges's flimsy objections would weary the reader and distract his attention from the main support upon which my study will stand, if it has the merit.

The question is whether the nett fidelity of surface modelling—so conspicuous in all axeheads on which corrosion has not gone too far—is capable of being produced by pouring metal into the bronze moulds. If it cannot, then Evans, in advancing his theory which is still so generally accepted, either did so without any knowledge of the *cire perdue* process, or confused it with sand casting technique.

The only practical experiment made on the lines of Evans's theory which I know of is the one which (more than 60 years later) produced the palstave cast which is now in the Pitt Rivers Museum, Oxford (see H. H. Coghlan, *Notes on the Prehistoric Metallurgy of Copper and Bronze*, Pitt Rivers Museum, 1951, pp. 112–15, cited in my first article). In my view, this cast by no means satisfies the surface requirement stated above.

A subsidiary question is: if Evans did in fact overlook the *cire perdue* process as the most likely explanation, how was it that it escaped his alert observation? I can only think of the following explanation.

From a conscientious examination of today's great wealth of bronze objects, made with an experience-conditioned eye—no matter how incredulous the observer—, certain facts emerge which may well have been obscure to Evans in those years of his enquiry prior to his publication in 1881.

Our contemporary collections of bronze reveal the tool-maker and the sculptor engaged upon the same technical problem—industrial and artistic workers sharing the common pool of know-how. The techniques of these two users have since drifted apart; the industrialist has gone into sand and the

sculptor has remained far more stationary in the *cire perdue*. The traditional sculptor is not, as Mr. Hodges supposes, on a hobby-horse pursuing archaeological pretensions, but is going about his proper business when scrutinizing all bronzes that come his way for traditional technique. In Evans's time *cire perdue* was more exclusively practised by quasi-industrial art bronze-founders whose technique was far more obscure to the public than it now is.

I am convinced that it is upon this surface difference that a solution to the problem depends, and not upon Mr. Hodges's observations, which place him among the few indeed who would credit corrosion with the designed and purposeful form covering the blade edge of the axehead from Ur.

Mr. William Fagg, Deputy Keeper, Department of Ethnography, British Museum, adds the following observations.—Ed.

I am not sufficiently versed in the European Bronze Age (my archaeological experience being in another field, for which bronze techniques are equally crucial) to understand Mr. Hodges's first point (or points), and I am not even sure what would be accepted as 'lowlands,' but I know a stone mould (for socketed axes) from Wiltshire in the Pitt-Rivers Museum, Farnham, and there is another, from the Fens, in the Manchester Museum.

Most ethnographers (and other technologists) would agree that steatite (point 2) is in brisk demand in many parts of the world because of the great ease with which it is carved, e.g. for making jewellery moulds.

On point 3, it does not seem sufficient to establish the occurrence on a pottery mould of copper, lead and tin salts, since these could presumably be attributed to the presence (or former presence) in the deposit of a bronze object. It may be relevant also to note that modern bronze-founders (and, I believe, some 'primitives') habitually use pulverized investment fragments as a 'grog' for inclusion in new investments.

During the past year I have examined a number of large collections of Bronze Age axes, looking for just such substantial flashes, or traces of their removal in the bronze, as Mr. Hodges mentions (5), but I have not found any, let alone a significant proportion; it is true that too many curators and collectors still have a weakness (I insist upon the word) for 'fine' specimens, and to specify (and publish) a sufficient number of these abnormal pieces would be a valuable contribution to pre-historic technology. I have, however, seen plenty of examples in which flashes have been removed without any evidence of working of the bronze.

It would seem incumbent upon Mr. Hodges to offer some alternative explanation for the seams, in the positions postulated in Mr. Underwood's reconstruction, which are evident on some surviving sprue cups in bronze, and notably on that illustrated in Mr. Underwood's third article (fig. 2d). His hypothesis of a core-forming technique similar to that deduced by Mr. Underwood for the bronze colossi of Sennacherib seems to me a somewhat desperate resort, which would hardly survive the inspection of interior casts taken from a few socketed axes (in which seams are always present). Since Mr. Hodges adduces the Benin technique, it may be well to make it clear that there is no evidence that the West Africans have ever departed from the 'one off' form of *cire perdue*; if they had developed, or needed to develop, a mass-production technique, they might well have evolved a sprue-and-runner system which would seem as ingenious and (at least to the non-technologist) as fussy as Mr. Underwood's reconstruction of the Bronze Age sprues.

It would be interesting to determine whether the waisted sockets of the axes from the latest Bronze Age (7) are also 'natural' in Mr. Underwood's sense. It seems to me that, on the assumption that Mr. Underwood's estimate of the drawbacks (if the pun will be excused) of the natural core is correct, it might have been so quickly abandoned that early Bronze Age examples would not be represented in our collections,



while the late examples might have been made possible by some improvement in technique such as the use of tampers (see fig. 2e in the third article), or simply by shortening of the axe body.

I was able to examine the Stibbard palstaves (8) when they were all together in the British Museum (to which only two of the looped specimens belong), and my observation entirely confirmed Mr. Underwood's. The question of 'short supply' of wax (9), which may have induced the economical technique of the hollow wax cast, seems to be a problem in the first place for the entomologist.

Lastly, I have carefully examined the very interesting shaft-hole axehead from Ur (12), and I cannot conceive that, if Mr. Hodges or anyone else were to inspect it, he could possibly hold to the view that the 'guard' ever formed part of the original head—whose edge indeed is clearly visible, intact except for superficial corrosion, through an opening in the forward edge of the 'guard.' This very well cast axehead has not suffered enough corrosion to obscure the rectangular section of its body, while the 'guard' has an entirely different and more improvised appearance. That butt-guard I would have to see!

# Motu Kinship Terminology. Cf. MAN, 1958, 153

**174** STR.—In his otherwise admirable book, *The Great Village: The Economic and Social Welfare of Hanuabada, an Urban Community in Papua* (London, Routledge & Kegan Paul, 1957), Dr. Cyril Belshaw has unfortunately published an account of Motu kinship terminology which is not entirely accurate. After giving the matter some thought I have decided that I must publicly join issue with him, for Motu kinship terminology is sufficiently interesting to merit an accurate report. In his book Dr. Belshaw deals with this subject on pp. 18–20 and on p. 270.

Motu distinguish between kinship terms when they are used in a 'strict' sense and when they are used in an 'extended' sense. My 'true fathers' (*Tamagu korikori*) comprise only my father, his brothers, and the husbands of my mother's sisters; but the total number of my 'fathers' in an 'extended' sense (all those whom I call *Tamagu*) may be very much larger.

The standard kinship terms used in their 'strict' sense (i.e. in a sense which entitles us to put the word *korikori* after them) are as follows:

Ego calls his or her siblings *Kakagu* (older sibling of either sex), *Tadigu* (younger sibling of either sex) or *Taihuagu* (older or younger sibling of opposite sex).

Ego calls his or her father, father's brothers and mother's sisters' husbands *Tamagu*; they call Ego *Natugu*.

Ego calls his or her mother, mother's sisters and father's brothers' wives *Sinagu*; they call Ego *Natugu*.

Ego calls his or her mother's brothers and father's sisters' husbands *Vavagu*; they also call Ego *Vavagu*.

Ego calls his or her father's sisters or mother's brothers' wives *Lalagu*; they also call Ego *Lalagu*.

Ego calls his or her grandparents or grandchildren *Tubugu*.

Ego calls his or her siblings' spouses or spouse's siblings *Ihagu* (or *Nakimi* or *Sibaga*, which are the equivalent terms in the Erema and Koita languages).

Ego calls his or her spouse's parents or children's spouses *Ravagu*.

The general rules of 'extended' Motu kinship terminology are as follows:

Ego refers to and addresses all cognates of his or her own generation as if they were siblings, calling them *Taihuagu*, *Tadigu* or *Kakagu*.

When Ego's mother calls any woman *Tadigu* or *Kakagu*, Ego calls that woman *Sinagu* and her husband *Tamagu*; they call Ego *Natugu*.

When Ego's father calls any man *Kakagu* or *Tadigu*, Ego calls that man *Tamagu* and his wife *Sinagu*; they call Ego *Natugu*.

When Ego's mother calls any man *Kakagu*, *Tadigu* or *Taihuagu*, Ego calls that man *Vavagu* and his wife *Lalagu*; they call Ego *Vavagu* and *Lalagu* respectively.

When Ego's father calls any woman *Kakagu*, *Tadigu* or *Taihuagu* Ego calls that woman *Lalagu* and her husband *Vavagu*; they call Ego *Lalagu* and *Vavagu* respectively.

Ego calls any cognate of his grandparent's or grandchildren's generation *Tubugu*.

When Ego calls anyone *Kakagu*, *Tadigu* or *Taihuagu*, he calls their spouse *Ihagu* (or *Nakimi* or *Sibaga*).

When Ego's spouse calls anyone *Kakagu*, *Tadigu* or *Taihuagu*, Ego calls them *Ihagu* (or *Nakimi* or *Sibaga*). They call Ego *Ihagu*.

When Ego's spouse calls anyone of the first ascending generation *Tamagu*, *Sinagu*, *Vavagu* or *Lalagu*, or anyone of the second ascending generation *Tubugu*, Ego calls them *Ravagu*. They call Ego *Ravagu*.

It remains for me to point out precisely where and how Dr. Belshaw has erred.

*Kakagu* does not refer to an 'elder sibling of the same sex' (p. 18), but to an older sibling of either sex. *Tadigu* likewise refers to a younger sibling of either sex. *Tadikakagu* is not a synonym for *Tadigu* (p. 18, p. 270); the former denotes the total body of my cognatic kin, the latter denotes my younger siblings or, by 'extension,' younger cognates of my own generation. Nor is *Nakimi* an equivalent for *Tadigu* (p. 18, p. 270); borrowed by the Motu from their Erema trading partners, it is used as a slang equivalent for *Ihagu*.

*Tamagu* cannot be properly translated as 'male relative of the father's generation' (p. 18); as Dr. Belshaw's table itself reveals, some male relatives of the father's generation, including the father's sister's husband and the mother's brother, are called *Vavagu*. *Tamagu* denotes only those men whom father calls 'brother,' or the husbands of those women whom mother calls 'sister.'

Dr. Belshaw's treatment of the terms *Lalagu* and *Vavagu* misses the crucial points. Those two terms are used with a precise and elegant symmetry. *Lalagu* denotes any woman whom father calls 'sister,' or the wife of any man whom mother calls 'brother'; or, if a woman is speaking, the term may reciprocally denote the child of any man whom the speaker calls 'brother,' or the child of any woman whom the speaker's husband calls 'sister.' *Vavagu* denotes any man whom mother calls 'brother,' or the husband of any woman whom father calls 'sister'; or, if a man is speaking, the term may reciprocally denote the child of any woman whom the speaker calls 'sister,' or the child of any man whom the speaker's wife calls 'brother.' (I use the terms 'brother' and 'sister,' though there are no equivalent terms in Motu, to avoid the repetitious use of the phrases 'sibling of opposite sex' and 'sibling of same sex'; the reader should bear in mind that those latter phrases more correctly translate the Motu terminology.)

It will be evident that one cannot reasonably translate *Lalagu* as 'female relative of the father's generation'; nor can one properly say that *Vavagu* means 'close relative through a female connexion,' or that *Lalagu* means 'similar relatives through male connexion' (p. 18). The terms are used far more precisely. After giving these misleading general translations Dr. Belshaw offers some more specific examples which come closer to the truth (p. 18). 'Mother's brother' and 'father's sister's husband' are indeed addressed as *Vavagu*, while 'father's sister' and equally (not 'optionally') 'mother's brother's wife' are addressed as *Lalagu*; but *Vavagu* can denote 'sister's child' only if a man is speaking, while *Lalagu* may denote 'brother's daughter' only if a woman is speaking (when it might equally denote brother's son, though Dr. Belshaw specifies only 'brother's daughter').

Curiously Dr. Belshaw does not list *Sinagu* at all in the list of kinship terms on p. 18 of his book. The four terms *Tamagu*, *Sinagu*, *Lalagu* and *Vavagu* are clearly of crucial importance.

The term *Natugu* is not applied to any 'relative one generation below speaker' (p. 18); some of these 'relatives' should be addressed as *Vavagu* or *Lalagu*, as Dr. Belshaw's table itself reveals. Furthermore *Natu lahaigu* (p. 18, p. 270) is not a kinship term at all, in the sense in which the other terms listed are kinship terms. *Lahai* is the adjective applied to 'extended' as opposed to 'strict' (*Korikori*) kinship usages. The correct Motu form, incidentally, is *natugu lahai*, not *natu lahaigu*.



*Ihagu* refers not only to spouses of cognates of one's own generation (p. 18) but also to those of one's spouse's cognates who are of his or her generation. *Tubugu advana* is not a kinship term in the same sense as the other terms given on p. 270; it is descriptive, meaning 'My grandchild's spouse,' whom I would address as *Ravagu*. (Similarly, if I should wish to state the precise relationship between myself and a man whom I call *Vavagu*, I might legitimately say that he is *Tamagu taihuna advana*, the husband of my father's sister, to distinguish him from other people whom I also call *Vavagu*.)

It is regretted that I have found it necessary to disagree with Dr. Belshaw on matters of plain fact about people among whom we have both done extensive fieldwork. I am quite certain, however, that the facts are exactly as I have stated them. I have both heard and used these kinship terms several hundred times a day in three Motu villages, including Hanuabada, over a period of nearly two years. In taking down dozens of genealogies I have always carefully noted by what term an informant refers to every one of his kin. My data contain nothing that does not accord entirely with the principles that I have set out here; those principles account for everything in my data; and in my data there are no ambiguities or puzzles that might explain how I and Dr. Belshaw have been led to offer such different accounts of the same subject.

MURRAY GROVES

Department of Pacific History, Australian National University, Canberra

'Gogmagog.' Cf. MAN, 1958, 111

175

SIR,—Can it be that Mr. S. S. Frere is really as simple as he makes himself out to be in his review of *Gogmagog*, or is he just a little bit of a naughty old deceiver?

Surely he must have heard the quaint story of what really happened to that C.B.A. Committee of Investigation: of how it split into two factions and how the learned trio that he mentions were responsible for a report which answered the wrong question and was backed up by air photographs taken in the wrong district on the wrong kind of geological formation. Naturally they got no marks at all for their answer, despite their brilliant exposition, a little like the wrapper of a patent medicine perhaps, of the benefits which attend their method of 'strip-tease' excavation. The other professor on the Committee who had different ideas was not permitted to voice a minority view, but sent in a private report answering the correct question in detail, and received full marks.

This is the sad story. Now, would it not be more dignified if Mr. Frere's heroes of antiquity, after three years of travail in chatter,

in due time producing this ludicrous mouse, were to adopt a scientific attitude of detachment and inquiry towards a problem which can only be solved by hard work? I have deliberately left the field clear for them to practise their subtle arts.

Branscombe, near Seaton, Devon

T. C. LETHBRIDGE

An Iron Mining Tool from Uganda. Cf. MAN, 1958, 40

176

SIR,—I regret that throughout my article I have carelessly referred to *enoni* as being chalk. The substance, being a fine white clay, is properly defined as kaolin.

My attention has recently been drawn by Mr. E. J. Wayland to an unpublished report by Mr. A. S. Taylor of the Geological Survey Department, Uganda, on an investigation in 1921 of the Ntanda group of shafts ('Report on Ntanda (with map and sections)'). Mr. Taylor recorded that the group covered about 40 acres and that 51 holes or shafts, varying from 4 to 34 feet in depth, were examined. Mounds of *débris* were noted near the holes. The deeper shafts had footholds cut in for descent and ascent. Kaolin occurred at about 11 feet below a covering of laterite. In all instances, according to the report, the kaolin had evidently been worked and extracted, the shafts being sunk through the clay into a deposit of lithomarge, the hydrous silicate of alumina.

It now seems highly probable that at Ntanda it was kaolin which was being sought with the possibility of some use being made of the deeper deposits of lithomarge.

Mr. Taylor does not report the finding of any mining tools.

Masaka, Uganda

E. C. LANNING

The Seligman Mask and the R.A.I. Cf. MAN, 1958, 124

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SIR,—The trustees and the staff of The Museum of Primitive Art have read with great interest your Hon.

Secretary's communication concerning the Benin ivory mask, formerly in the collection of Mrs. Brenda Z. Seligman, and now in this Museum. As an institution devoted to the study of the aesthetics of primitive peoples, and the fullest enjoyment of their art (against the widest background of understanding), we share Mrs. Seligman's belief in the utility of the apparently useless. We are proud to have and to show the ivory mask. It will go on exhibition in the autumn, and we are confident that its attraction at that time will destroy any resemblance this Museum may have to an ivory tower.

ROBERT GOLDWATER

The Museum of Primitive Art, New York

Director

## REVIEWS

### GENERAL

**A Natural Science of Society.** By A. R. Radcliffe-Brown, with a foreword by Fred Eggan. Glencoe, Ill. (Free Press) and Indian Hills, Colorado (Falcon's Wing Press), 1957. Pp. xii, 156. Price \$3.50

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**The Theory of Social Structure.** By S. F. Nadel, with a memoir by Meyer Fortes. London (Cohen & West), 1957. Pp. xvi, 159. Price 18s.

The simultaneous publication of these two posthumous volumes is both a symptom and a portent. Taken together they perhaps mark the beginning and the end of a special phase in the development of social anthropology. The two books are strictly comparable. Both originated in a course of University lectures, both are concerned with the general theory of social structure at the highest possible level of abstraction. But, in origin, they are separated by nearly 20 years.

The Radcliffe-Brown volume is a *verbatim* transcript of lectures delivered at the University of Chicago in 1937. A restricted duplicated edition of the same text has circulated since 1948 under the title *The Nature of a Theoretical Natural Science of Society*. The theme was the author's favourite topic of discourse and this fact is reflected on every page. Each sentence has a gem-like polish, every statement takes the form of an all-embracing generalization. But the sense is aphoristic rather than exact; through repeated repetition, all facts

of ethnography or history have become moulded to what Radcliffe-Brown believed must necessarily be the case. Such beautiful lucidity is entrancing; all the more reason to ask: does the acknowledged merit of 1937 still endure 20 years later?

Radcliffe-Brown's general viewpoint is today sufficiently well known. Human societies throughout the world differ in their systemic organization. Systems can be classified according to their structure. The taxonomic classification of social structures is a scientific end in itself and constitutes the first essential step towards the formation of a 'science of human society,' the exact nature or purpose of which is left conveniently obscure.

The book presents an elegant façade of widely ranging scholarship. The author plays by turn the roles of historian, physicist and mathematical logician but always with one central aim in view. His book is a plea for the scientific utility of a comparative taxonomy of total social structures. Although, in practice, these totalities are much more difficult to identify than Radcliffe-Brown was prepared to admit, this is a gospel which has had extremely important consequences for social anthropology. At least four well-known symposia<sup>1</sup> are directly in line with the doctrine here laid down.

Yet one may wonder. Why, after all, should similarity of 'structure'—if we are certain what we mean by that—be scientifically so important? Might it not be as well to take warning from the



botanists? In that field, over the past two centuries, the concept of 'species' has been laboriously constructed on purely structural principles but it now transpires that, in the most significant of all botanical enquiries, that of cross-breeding and reproduction, structurally defined species are not specific at all! Botanists still resort to classification but the principles on which this classification is based are no longer exclusively structural.<sup>2</sup>

The following quotation from a botanist,<sup>3</sup> if taken to apply to social anthropology, is in direct contradiction to Radcliffe-Brown's thesis:

'Comparative morphology cannot in fact exist as an independent scientific discipline: it must make the fullest possible use of the data and conclusions of causal morphology, which in fact involves physiology, genetics and ecology.'

Doubts of this general kind seem to have been present in Nadel's mind when he wrote *The Theory of Social Structure* for he is plainly sceptical concerning the scientific value of 'a structural frame of reference' unless it can somehow incorporate 'concepts connoting purpose and utility' (p. 158).

Nadel's book is an enlarged version of a course of lectures delivered at the London School of Economics in the spring of 1955. At the time of his death the text was already in proof but the absence of an index and one or two blatant errors (e.g. p. 121) in the 'mathematics' with which some chapters are liberally bespattered suggest that final revision was still lacking.

Although Professor Fortes, at the end of his interesting memoir, expresses the opinion that this will become 'one of the great theoretical treatises of twentieth-century social anthropology' my own view is much less optimistic. Those who are not devotees of the subject are likely to find the book unreadable. Nadel seems to have aimed at doing three quite separate things. First, he examines in great detail the implications of 'role theory' which has found its way into social anthropology chiefly through the writings of Radcliffe-Brown, Linton and Max Weber. Secondly he attempts, towards the end of his book, to arbitrate between the diverse views of social structure advanced by a number of different social anthropologists, notably Radcliffe-Brown, Firth, Fortes, Lévi-Strauss and myself. But thirdly he has used his book as a platform from which to propound his highly personal view that the future of abstract anthropological theory lies in the development of an appropriate calculus of symbolic logic. It is this last feature which is disastrous.

Every now and then the argument is interrupted to present a series of generalized propositions expressed in a symbolic logic of the author's own devising. None of these symbolic statements have any meaning until Nadel has himself explained them in his accompanying text and none of them lead to conclusions which are not much more readily propounded in simple English. The only positive effect of this excursion into 'mathematics' is to deter the reader from trying to understand the argument.

Those who are tough-minded enough to ignore the calculus will find the discussion of rôle theory very stimulating. There is much here with which I personally do not agree—e.g. the notion that societies can be usefully distinguished according to the number of different rôles which they contain (pp. 61f.)—and unfortunately the most personal and original section (pp. 115–21) is the worst entangled with symbolism. Who on earth is going to work his way through the following?

'if  $E: A[\geq(crb)]$  is such that  
 $ErA \supset E(ca)A$   
 $\therefore ErA \rightarrow A[\geq(crb)]$ '

Even with the clues that  $ca$  stands for 'command over the actions of others' and  $crb$  for 'command over services and benefits' the reader is not likely to be much the wiser. But the cross-cutting taxonomy of rôle types (summarized in the charts at pp. 53, 73) and the integration of this taxonomy with the distinction between 'leadership rôles' and 'expressive rôles' which appears in the work of Parsons and Bales<sup>4</sup> marks a genuine step forward.

Yet in the outcome, my total reaction is one of scepticism. For Radcliffe-Brown, it was a methodological dogma to assume that 'a social system is not purposive.'<sup>5</sup> Dismayed at the implications of such detachment, Nadel would have us abandon the carefully formulated distinction between rôle and status (p. 109). Despite his

fondness for mathematics Nadel still wants to stick closely to the empirical facts; an office, it seems, has no existence apart from the individuals who fill it. In fact Nadel does not really admit that there is anything abstract about social structure at all. 'Empirical constancies do exist and are observable' (p. 145). Perhaps. But do anthropologists in fact observe them? I cannot help remembering that that excellent volume *The Nuba*,<sup>6</sup> which records a very large number of structural constancies for no less than 10 different tribes, is based on empirical observations covering about 12 months.

I think that both these books deserve to be read and read together, for they pose a question. Has the too narrow pursuit of Radcliffe-Brown's principles led to a dead end? Was Nadel pointing the road ahead? I myself would answer the first question with a 'yes' and the second with a 'no,' but that is just a matter of opinion.

E. R. LEACH

#### Notes

<sup>1</sup> Fred Eggan (editor), *Social Anthropology of North American Tribes*, Chicago, 1937; M. Fortes and E. E. Evans-Pritchard (editors), *African Political Systems*, London, 1940; Fred Eggan, *Social Organization of the Western Pueblos*, Chicago, 1950; A. R. Radcliffe-Brown and Daryll Forde (editors), *African Systems of Kinship and Marriage*, London, 1950.

<sup>2</sup> H. J. Lam, 'Classification and the New Morphology,' *Acta Biotheoretica*, Leiden, 1948.

<sup>3</sup> C. W. Wardlaw, *Phylogeny and Morphology*, 1952.

<sup>4</sup> Talcott Parsons and R. F. Bales (and others), *Family Socialization and Interaction*, Glencoe, Ill., 1955.

<sup>5</sup> *A Natural Science of Society*, p. 155.

<sup>6</sup> S. F. Nadel, *The Nuba*, London, 1947.

**The Dentition of the Australopithecinae.** By J. T. Robinson. *Transvaal Mus. Mem. No. 9. Pretoria, 1956. Pp. 179, 50 text figs.*

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We must all congratulate Dr. Robinson on this publication which for the first time gives a complete description of the dentition of all known Australopithecinae. The material studied consists of no fewer than 448 teeth of the permanent and 78 of the deciduous dentition; of these 273 and 38 respectively come from Swartkrans alone. Robinson's description gives an excellent and detailed review of the finds, illustrated by photographs, which show many of the details much more clearly than the drawings in the older publications. The human affinities come out most convincingly (in 1945 Simpson still regarded the Australopithecinae as Ponginae). A surprise is the low dental pulp cavity (fig. 5), which is not taurodont as in most early hominids.

The African Australopithecinae are classified by Robinson as follows:

Superfamily: Hominoidea

Family: Hominidae

Subfamily: Australopithecinae

Genus: *Australopithecus*

Species: *A. africanus*

Subspecies: *A. a. africanus* (Taungs)

*A. a. transvaalensis* (Sterkfontein, Makapan).

(Incertain sedis: *Australopithecus* from Laetoli beds, East Africa. Resembles *A. a. transvaalensis* on the scanty available evidence, specific status uncertain).

Genus: *Paranthropus*

Species: *P. robustus*

Subspecies: *P. r. robustus* (Kromdraai)

*P. r. crassidens* (Swartkrans)

*Telanthropus* from Swartkrans 'is transitional between the pre-hominid and euhominid grades and can, most probably, best be placed in Euhomininae' (p. 11). *Meganthropus palaeojavanicus* from Sangiran, Central Java, is also included by Robinson in the genus *Paranthropus*. This is incorrect, as the Javanese form differs essentially from the Australopithecinae in the size relations of the premolars, of which the last one has only a single root (as *Pithecanthropus*, *Sinanthropus* and the other Euhomininae), while the same tooth in the Australopithecinae has always two roots as in the Pongidae.

The clearest way to distinguish the species and subspecies is the morphology of the first deciduous lower premolar. On account of



this Robinson separates *Australopithecus* and *Paranthropus* as two branches, a long-standing division, but they only differ in the degree of molarization: those of *Australopithecus* are less, those of *Paranthropus* completely molariform. According to the morphology, Sterkfontein is the most primitive, then follows Taungs, then Swartkrans, while in Kromdraai we find no difference between the first and the second deciduous molar except for the size, so this must be the youngest form. This morphological sequence is so completely in agreement with the stratigraphical dating of the sites by means of the fossil carnivora by Mrs. Ewer (*Proc. Zool. Soc. London*, 1956, p. 270), that it seems difficult to suppose two sharply separated lines of evolution. Also the more primitive forms have, as might be expected, the more strongly developed canines (Sterkfontein). The late Father Teilhard's impression, that the *Australopithecine* form 'a single natural group, caught in a phase of rapid evolution,' seems to me still the best way of interpretation, in spite of Robinson's protest (p. 171).

About the relationship between the *Australopithecine* and the *Euhominina* Robinson comes to the following conclusions: '*Telanthropus* gives a clear indication of how an *australopithecine* of the *Australopithecus* sort could have been converted into a *euhominid*. *Telanthropus* itself may not be precisely in the line of descent of *euhominids*, but must have been so close to such a direct ancestor as to make no essential difference, containing as it does so much of a mixture of *prehominid* and *euhominid* characters. From this stage the main outlines of *euhominid* evolution are fairly clear, passing through the so-called early Neandertals, with the *Pithecanthropus* group and the classic Neandertals as offshoots of the main stream' (p. 172).

Is, one is tempted to ask, *Telanthropus* sufficiently known to warrant such a statement? And what valid reasons are given to put the *Pithecanthropus* group on a sideline? While, judging again from the first lower deciduous molar, 'The *australopithecine* condition is . . . as molarized or even more so, than the most molarized form of *euhominid* tooth' (p. 158). Would this (and other observations) not merely suggest an unknown common Pliocene ancestor for the *Australopithecine* and the *Euhominina*, as was formerly proposed by the late Dr. Broom (1945)?

We must be grateful to Dr. Robinson for the clear representation of the material; that certain phylogenetical points are still open to discussion is only natural. His publication is the most important contribution since Weidenreich's memoir on the dentition of *Sinanthropus*.

G. H. R. VON KOENIGSWALD

## Das Pferd in prähistorischer und früher historischer Zeit.

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By F. Hančar. *Wiener Beitr. Kulturgesch. Ling.*, Vol. XI. Vienna and Munich (Herold), 1956. Pp. 651, 30 plates, 63 tables. Price DM. 62.

This book of 651 pages contains an enormous wealth of material on the horse. Reindeer and cattle are discussed also, especially in relation to the horse. Abundant use is made of tables, and a very large one giving the chronology (No. 12) will be useful to those who know the pitfalls of this subject.

As a mine from which information can be obtained by means of mental excavation, Hančar's work will always remain important. His knowledge of the archaeological literature of Russia and other eastern countries is unique, and many summaries of inaccessible publications are for the first time made available in his text. The synthesis, however, suffers from two drawbacks. First, one gains the impression that the author is apt to get drowned in the mass of his own material. And secondly, his German is so complicated that many statements require grammatical analysis before they become clear. His sentences are extraordinarily involved and long, 10 to 15 lines being common. The longest I have discovered is 31½ lines. This makes it quite unintelligible, even in German. I have tried to translate passages into English without success.

According to Hančar, in the first stage of economic exploitation the horse was providing meat. It was incorporated in the domestic stock late in the third millennium B.C. from the Dümmer Lake in Germany to the Altai Mountains. The second stage began when its use as a means of transport was discovered at the end of the third millennium, when it replaced cattle and hemionies in many places. The horse became a really important beast of transport with the great western thrust of chariot-using tribes which reached Egypt about 1600 B.C. Riding enters the picture only from about 800 B.C. onwards (on the evidence of the Mycenaean rider described by Sinclair Hood, however, it had started by c. 1300 B.C.).

The chronology of the sites is in many places less secure than the author assumes. For the Tripolye culture, for instance, he adopted the older long chronology, evidently being unaware of correlations with Danubian phases that have since been established.

Under the title 'Kulturelement Pferd: Wertung und Einbau' (Cultural element Horse: Valuation and Incorporation), the author has published an 11-page summary of his own book in *Saeculum*, Vol. VII, No. 4.

F. E. ZEUNER

## AFRICA

**West African City: A Study of Tribal Life in Freetown.** By Michael Banton. O.U.P. for the Internat. Afr. Inst., 1957. Pp. 221. Price £1 15s.

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In this study of urbanization in Freetown Dr. Banton has set himself a threefold task—to examine the causes and character of migration to the town, the development of an effective system of urban administration in an immigrant and heterogeneous community, and the way in which tribal institutions adjust and are adapted to the new environment. To begin with, he describes the conditions under which Freetown emerged and some of the social and administrative problems that have arisen as a result of the large-scale immigration of tribesmen from the Protectorate, particularly since the war. This leads on to a discussion of the factors making for migration from the rural areas, and of the avenues of employment available in the towns to which the migrants are attracted. Here Banton has been able to use some of his own material collected on brief visits to a number of villages, but the ground he attempts to cover is too wide, and the material too thin, to allow of any very detailed and systematic analysis of the problem.

In the third part of the book we return to Freetown, and are given some account of the population of the town, and its different ethnic, tribal and religious groups. However, the sections which will be of most interest to students of African urban society are Dr. Banton's accounts of tribal headmanship and the 'compins,' which in many of their features bear a striking resemblance to the tribal dance teams of the Northern Rhodesia Copperbelt described in Professor Mitchell's *The Kalela Dance* (Manchester U.P., 1957). Banton

analyses the 'compins' in terms of their function in re-integrating the tribal social system (of the Temne). This may be so in the sense that the younger Temne now express a pride in membership of the tribe and no longer seek to move out of it, but some may doubt whether this is the most useful way of approaching the 'compins' when so many of their officials 'represent' personalities prominent in the wider social structure, when the dances themselves are not traditional, when so many features of the 'compins' activities emphasize European or Creole-derived values, and when the members themselves—unfortunately Dr. Banton does not tell us anything of the actual social composition of any of the teams—'shared common values appropriate to a semi-industrial society and partially opposed to the traditional order.' There is a paradox here such as Mitchell observed in his analysis of the Kalela dance, and it is not readily resolved by reference to the concept of tribal re-integration.

*West African City* suffers from a number of faults for which the author is probably not wholly responsible. Limited by time and financial resources, his terms of reference have led him to try and cover too much ground, and the book suffers in consequence an inevitable raggedness. Banton himself takes the view that to study 'detribalization' it is necessary to isolate the system of relationships within a given group, and to analyse the changes within that group. Whether one accepts this view or not, a fully systematic analysis along these lines of social relations, say among the Temne, and of their place in the wider community, might have given us a deeper insight into the workings of Freetown's social system.

A. L. EPSTEIN



**Rituals of Kinship Among the Nyakusa.** By Monica Wilson. London (O.U.P. for Internat. Afr. Inst.), 1957. Pp. 268. Price £1 15s.

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This is the first of two volumes describing the symbolism and functions of Nyakusa rituals. The present book deals with rites associated with death, birth, puberty, marriage and abnormal occurrences in family life such as twin births, murders, suicides or sickness caused by vengeance—ceremonies in which kinsmen participate in groups as agnates, affines, lineage or clan members, and which are of importance to the maintenance of the kinship system. The second book, which is to be published shortly, will deal with rituals of chieftainship. Professor Wilson will then have given us the bulk of her own and her late husband's material collected by both during the years 1934–38 and by herself alone on a recent trip in 1955. She has also worked out in full the theoretical approach to the study of ritual which has been a dominant interest of both these anthropologists since the start of their work. (Cf. Godfrey Wilson, 'An African Morality,' *Africa*, 1936; 'Nyakusa Conventions of Burial,' *Bantu Studies*, 1939; Godfrey and Monica Wilson, *The Analysis of Social Change*, 1945.)

This is one of the most careful studies of primitive ritual which we have. Both the Wilsons must have been fine linguists, with a power of close contacts with their informants and with unusual energy and precision in the recording of expressions and phrases. They also had the theoretical preoccupations which alone make possible such a standard of observation.

The symbolism of Nyakusa ritual is unusually complex, but Professor Wilson is concerned to prove that ceremonies which consist of a multitude of small ritual acts yet express a common body of ideas, or an attitude to the world, and that the acts themselves conform to a 'symbolic pattern.' The pattern which she presents to us is mainly derived from the comments of specially thoughtful informants, constantly pressed by the Wilsons 'Why?' (They give their questions as well as the answers.) To this is added the results of a patient teasing-out of meanings through a comparison between similar symbols used in different rites, after the method used by Radcliffe-Brown in a much more superficial analysis of Andaman Islands ritual as long ago as 1922.

Nyakusa use as symbols objects of emotional interest in their particular background such as the plantain which is the symbol of the male and the sweet banana which represents the female; but these, and many other symbols, connote ideas which are basic to Nyakusa philosophy and to their attitudes to life, death, fertility and the life after death. As in other Bantu societies ancestral spirits, or 'shades,' are thought to return in dreams and to have powers over the health of their descendants; but here they are definitely believed to be responsible for potency, the fertility of women and crops, sex desire and semen. They are symbolically associated with all objects connected with the sex life of man and wife, the hearth, the fire, the firewood, the bed, the hut door. They haunt the family banana groves where a root represents the lineage constantly replacing itself. But shades are also filthy, fearful, bringing danger of madness and they must be constantly driven away. Those temporarily 'brooded over' by the shades, such as mourners, do filthy things and are subsequently purified by washing, throwing away symbolic objects, burning or sex intercourse of husband and wife, which is the climax of each rite. Death is removed by a war dance which turns into a dance of sex prowess and sex display.

Professor Wilson's analysis is stimulating to other students of Bantu ritual, for taboos surrounding the hearth and ceremonies for 'removing the death' from the partner of a dead man or woman are widespread in these societies. The connexion between fire, sex, semen and the power of the ancestral spirits may well be implied in the *chisungu* ritual of the Bemba which I have recently described (*Chisungu: A Study of Girls' Initiation Ceremonies in North-East Rhodesia*, 1956), although such associations were never made in my hearing—perhaps because my questions were less patient and insistent than the Wilsons'.

The author has had many difficulties of presentation with which other ethnographers will sympathize. Her material was unusually full, collected as it was by two skilled observers and by two African clerks. The anthropologists attended 30 funerals between them. It

comes from two groups of Nyakusa who are culturally distinct. The social and economic background necessary to an understanding of such rites she keeps to a minimum, but eyewitness accounts of ceremonies, and texts giving Nyakusa statements are essential to the purpose even though this involves some repetition in the case of the two areas, for she is concerned to prove that common ideas exist in both although they are expressed in rather different idioms. Some will find the detail heavy, but I see no way out. Ethnographical data must be full at the beginning of a new level of analysis. Generalizations can only be made when a number of books like the present have become available. Some 15 or 20 years ago anthropologists not specially interested in the study of kinship structure found the constant pre-occupation with genealogies, which was then fashionable, rather wearisome; those not specially concerned with symbolism may find some of Professor Wilson's material too full. Both types of case material seem to me to be essential at this stage of our work.

It is to be hoped that psycho-analysts will comment on this book. Psychologists have often complained that they could not use anthropological material because it was not fully enough documented and contained too much of the anthropologists' own interpretation; but some of the native texts here given approach in detail psycho-analytical case histories and it would be of the utmost interest to know if they can be used by psycho-analysts. This raises the perennial question of the type of co-operation possible between psychologists and anthropologists—a question not yet solved in this country, at any rate. It would have been impossible for these data to be collected without a very full knowledge of the language, the social structure and the emotional pre-occupations of the Nyakusa. Are the psycho-analysts willing to acquire a new discipline and new skills or is it possible for the anthropologist to present them with material in a form which they can use? A. I. RICHARDS

**The Chiga of Western Uganda.** By May M. Edel. London (O.U.P. for Internat. Afr. Inst.), 1957. Pp. 200. Price £1 15s.

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Dr. Edel studied the Chiga in 1933; but until this book we have had to rely on an occasional paper from her and on a scrappy account from Roscoe for information on this people. They are ethnographically interesting as being the only people of any size in the Interlacustrine Bantu area that escaped the overlordship of the Hima-Tusi. They live in high mountainous country, not ideal for cattle and difficult of conquest; and although they have been subjected to pressure from the neighbouring Ruanda and Ankole and to raids by what Dr. Edel refers to as 'a terrible Pygmy army,' they managed to retain independence until the coming of the British.

This is a straightforward and conventionally arranged ethnographical account of Chiga culture. Various aspects are considered in turn, making up a rather barren ABC of the life of a single small community on the edge of Lake Bunyonyi. There is little comparison made between this and other Chiga communities, and we do not know how typical it is. The account is flat, being little more than a mere listing of details of custom, and with no particular frame of reference to give form to the whole. Chapters are on Social Structure, Kinship, Marriage, Economics, Social Control, Religion and Education. The first is presumably meant to give us an outline of total Chiga society and a basis for the rest of the account, but if this was the intention it does not succeed. Admittedly, in 1933 little was known of the structure of African political systems, and Dr. Edel took the segmentary lineage system and the lack of centralized authority as signs of social disintegration; this view was supported by the almost complete lack of religious practices. Despite the use of 'structural' jargon, we are told very little about the structure of Chiga society. Almost every chapter is superficial and too generalized. We are told repeatedly and naïvely that Chiga culture is typically 'African'; that does not make up for the lack of detailed data necessary for a proper understanding of the society. There are almost no texts, no detailed descriptions of events and few actual examples of the items of behaviour catalogued. The difficulty, of course, is that we expect too much today from an account of research done in 1933.

The best chapter is that on religion. Chiga religion had been



almost totally banned by the Government, which wished to destroy the Nyabingi cult, originally a response to Ruanda threats of invasion and later to European rule. Here we are given straightforward accounts of what actually happened, as far as Dr. Edel could discover, and the book begins to come to life.

The main weakness of the book is in its conventional 'cultural' approach; it is also published 20 years too late. Its main strength is that we have here a total account (even if only a preliminary one, as Dr. Edel herself says), of an ethnographically important people.

JOHN MIDDLETON

**When Egypt Ruled the East.** By George Steindorff and Keith C. Seele. 2nd edition. Chicago (U.P.) (U.K. agents: C.U.P.), 1957. Pp. xviii, 289, illustrations, plans and maps 110. Price £2 3s. 6d.

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This synopsis of the most brilliant period of the history of Ancient Egypt is a welcome addition to the number of popular books on that great civilization. The profound knowledge of all the documentary evidence of the period, which these two eminent Egyptologists possess, makes all the historical part authoritative, and the easy flowing style of the translations brings to the mind and the imagination of the reader a vivid picture of the persons whom those documents record. Thus one can almost see Queen Hatshepsut giving peace to the country after the turmoil of civil war, and laying the foundations of its future prosperity by her encouragement of trade; that military genius, Thutmose III, adding province after province to his empire; Amenhotep the Magnificent, the perfect type of the oriental Sultan; and Akhenaten, the religious fanatic, who in his zeal for his god neglected his duty to his people and brought his country to the verge of ruin. Fortunately the authors did not confine themselves to Egypt only, but have devoted one chapter to the 'Outside World,' giving a very clear and interesting account of the countries with which Egypt was in touch in the north, south and east. There is some small archaeological evidence for Egyptian contacts with the

west, but nothing documentary. The nineteenth dynasty suffers by contrast with the dazzling splendour of the eighteenth, and this is seen in the chapter on this period. Yet one cannot help feeling that had the authors made as detailed a study of the latter half of the great period as they did of its more brilliant half they could have produced a chapter of enthralling historical interest. I would like to suggest to the surviving author that this still remains to be done.

The historical chapters include a brief account of the rise of Egypt from its most primitive to the high civilization of the New Kingdom. At the close the Fall of Egypt is given shortly but adequately. Egypt ended when Rome overran the known world and forced her form of civilization on the subject nations. The bulk of the book is concerned with the eighteenth dynasty; not only with the history but with a survey of the civilization of the period. The method of writing, the literature, the science, the art, and the religion are passed in review. The science of the Egyptians is unfairly contrasted with that of the Greeks, for the Greek civilization was a thousand years later. The chapter on art includes statues, painting and architecture.

But in dealing with the civilization the authors show that they have no knowledge of anthropology, especially of early religion, or even of any religion that includes a plurality of gods. Two quotations will suffice to show their attitude towards the religion of ancient Egypt: 'It certainly should not have been too much for a clever brain to have constituted some sort of order out of this mixture of diverse mythological ideas' (p. 143); 'The Egyptians failed no less completely to achieve a consistent set of ideas regarding man's destiny in the life after death' (p. 144). One wonders if the authors have ever realized that that highly civilized land of India still has a pantheon as large in proportion as that of ancient Egypt? If a future edition of this otherwise excellent survey of the peak of ancient Egyptian civilization is contemplated, it would be good to have the archaeology and anthropology brought up to date.

M. A. MURRAY

## EUROPE

**Rio De Onor: comunitarismo agro-pastoril.** By Jorge Dias. Porto (Instituto de Alta Cultura, Centro de Estudos de Etnologia Peninsular), 1953. Pp. 610

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To many valuable studies in Peninsular folklore and ethnography Senhor Jorge Dias has added this detailed study of a somewhat unusual village life. This is a double village, on the upper waters of the Açar—Rio de Onor on the Portuguese and Rionor de arriba on the Spanish side of the modern frontier. The inhabitants, in whose culture the author detects a pre-Celtic element, are trilingual; they possess large numbers of goats, sheep and cattle, and are noted for their gaiety and conviviality. They dance away their leisure hours; and the village council, holding session in the open air, drinks on the spot the fines (in kind) which it imposes.

Grazings, meadows and ploughlands are common property administered by the general assembly and cultivated by households, though the law requires them to be registered under the names of individual taxpayers. Village organization hinges on the water meadows, since no one is a member of the assembly unless he has livestock there.

In fact, a principal function of the council and the two *môrdomos* is the management of the water meadows, though they also administer justice, and have lately bought some farm machinery. The social unit is the household, occupied by the extended family; and the arable land (for rye) is held by the households in theoretically equal shares and redistributed from time to time. This, with the necessity of stinting the water meadows, requires that the number of persons in households shall be kept stable, and it was the rule, until after the First World War, that men should not marry until their parents were too old to work. New ideas learnt on active service, and the introduction of chemical fertilizers, making it profitable to break up waste land, have encouraged some young men to marry and set up separate households; but these are not represented in the village assembly.

The technological chapters are ample and well illustrated: the large wheelcart, thought to be the most archaic in the Peninsula, is treated in great detail. The plough is of the type called by the author *radial lusitanico*, i.e. with the open sloping posterior curve characteristic of extreme north-eastern Portugal. The folklore section is rich, and the author's daughter contributes a *cancionero*.

BARBARA AITKEN

**Otok Susak.** Zagreb (Jugoslavenska Akademija), 1957. Pp. 586, plates, some in colour

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This is a detailed study of the island of Susak (barely one-and-a-half square miles, 1,433 people living in a village) in the Lošinj group off the Illyrian coast, south-east of Istria. It is without cattle or donkeys or, now, sheep or goats and the loose sandy soil is protected from erosion by terracing and by planting reeds, willows, etc. In the last 200 years vines have come to occupy three-quarters of the surface and sale of grapes and wine have given some prosperity leading to improvement of housing from the old dry-stone chimneyless huts surviving from pre-historic times. All portage is human and all work was by hand until a fish-preserving factory with electric power was established. Egalitarian tendencies are very strong on a basis of subsistence cultivation; fuel and water are both scarce. Twenty to thirty per cent of the people's income is in gifts from emigrants to America; there is a large surplus of women on the island, several having husbands in the U.S.A. or on the mainland. Clothes and goat-skin shoes are home-made from imported materials, changes of fashion are practically unknown. The spirit of collectivity is emphasized in many ways. A good deal of inbreeding occurs but is apparently not close enough to show special results. Blood-group percentages are: O, 46; A, 49; B, 4; AB, 1. Birth rate (33.4) is declining, as is the death rate (23). Summaries of sections of the book are given in one or other of the west European languages.

H. J. FLEURE





**A TERRA-COTTA HEAD EXCAVATED AT IFE, 1958**

THE ROYAL ANTHROPOLOGICAL INSTITUTE CHRISTMAS CARD, 1958

*This head, about two-thirds of life size, and doubtless originally part of a complete figure, is the finest of those excavated in January this year by Mr. Frank Willett for the Nigerian Antiquities Service at the rich site of Ita Yemoo on the outskirts of Ife, Western Nigeria. It was at this site that the chance discovery had taken place in November, 1957, of the bronzes illustrated in MAN for January, 1958 (Plate A). The head is of undetermined sex, though a somewhat similar headdress is worn by a female bronze figure from the same site.*





THE 'IDENA' SHRINE IN THE ORE GROVE, IFE

*Photographs: F. Willett*



# THE ORE GROVE AT IFE, WESTERN NIGERIA\*

by

K. C. MURRAY, O.B.E.

*Nigerian Antiquities Service*

and

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**187** The Ore Grove at Ife,<sup>1</sup> known as Igbo Ore, lies on the right side of the Ifewara Road, about 300 yards beyond the town wall. It has been described several times, for it is about the only grove in Ife which Europeans are taken freely to see. Dennett<sup>2</sup> gives a plan of it, which does not correspond, however, with its present arrangement, and some diagrammatic sketches of the standing stones. Frobenius,<sup>3</sup> whose visit must have been a year or two later, gives a very detailed description, and claims: 'The English had previously known of these places, but had not come across anything interesting because everything that was so had been concealed beneath heaps of straws and matting. Of course, we at once set to work with all the "German thoroughness" at our command, unearthed all sorts of extremely noteworthy objects, such as stone vessels, sculptured stone stools, stone crocodiles, and finally also the monument called the Idena, all of which had here lain concealed' (p. 86). Talbot<sup>4</sup> and to a less extent Hambly<sup>5</sup> give an account of the carvings, as also do Bertho and Mauny.<sup>6</sup>

At one time there was a market in front of the grove and there are said to be still at this point traces of paving on the Ifewara road. The path to the grove is marked by *peregun* trees (*Dracæna fragrans* Gawl.) and a short way along it are four thin upright stones dedicated to Ogun, the *orisha* (god or spirit) of iron. The fourth stone must have been placed there recently, for Dennett (p. 19) only mentions three and there were only three in 1936. Talbot gives a photograph of the three stones (Vol. II, fig. 90). Frobenius calls them 'a group of quite coarse little pillars of stone about waist-high. They are angular, roundish, and, at all events, roughly hewn or chipped off, absolutely bare of any detail.' He mentions another stone which is probably the fourth stone that has now been added to the group: 'Going forward we come to another, rather more to the left.'

Some yards beyond the group the path divides. The left branch leads to a very small clearing. 'Here . . . rises a stone image. This is about thirty-two inches high, roughly executed and defaced. It has one chain round its neck and another hangs over an apron skirt down to the hands folded over its stomach. On its left side it has a peculiar hanger, something like the tassels of a Houssa sword' (Frobenius, Vol. I, p. 296). Dennett and Talbot do not add to this description except to name the figure Ore (Awre), and Talbot mentions armlets on the wrists and 'a hole in the forehead, said to have been caused by a bullet

fired against him by rebels' (Vol. II, p. 339). He also says that the hair is represented by iron pegs inserted into small holes, but he has clearly confused this carving with the Idena statue described below. On various visits I (K.C.M.) have been given different names for this figure, but Olopo, the priest in charge of the shrine, said it was Olofefunra, the servant of Ore.<sup>7</sup> Frobenius gives two drawings of this figure (Vol. I, p. 297, fig. 2) and Talbot two photographs (Vol. II, figs. 83, 84), showing it under a hut which is not necessarily rebuilt each year (see fig. 1). Bertho gives a photograph (his fig. 10) and records it as being Oke-Ore, the husband of Ore, whilst Mauny was told that it was Idena (p. 102).

The carving leans against a broken stone stool, and in front of it is a small worn slab, broken in two, ridged on the underside as if it is a lid. The top bears a carving in relief of a snake catching a frog or lizard (see fig. 1).



FIG. 1. STONE FIGURE OF ORE (?)

\* With Plate L and four text figures



Mr. Newberry, Agricultural Education Officer, says that he saw the two pieces separate and fitted them together. His guide had not noticed that they belonged to the same carving. On the left of the figure is a large stone slab apparently of quartz leaning out of the ground like a tomb-stone. Dennett mentions this stone (though Frobenius does not), and says that near to the figure were 'two kola boxes or dishes in stone each one foot long and six inches wide' (p. 21). Frobenius (Vol. I, p. 297) mentions the stool, which is shown in his drawing of the figure as cracked but complete, but only one dish. 'This, the first specimen of quartz work I had seen, claimed my special attention. It was a four square block, whose upper surface had been chiselled to a depth of about one and a quarter inches, while the lower one had a raised ring on which it stood' (Vol. I, p. 296). A lizard was carved on its side (illustrated in Vol. I, Plate III, facing p. 304).

The right-hand path leads to another and better carved figure called Idena by Olopo (see Plate L). Frobenius gives it this name also, but Talbot (Vol. II, p. 339) calls it the slave of Ore, *Gbanna Awre*, while Dennett (p. 22) calls it the wife of Ore. Bertho was told that it was a woman called Ore, but Mauny was told that it was Rere, the wife of Ore (p. 102). Frobenius (Vol. I, p. 298) describes the carving: 'it was housed in a badly damaged little hut whose thatch almost hid it. It is a granite figure about thirty-six inches high above ground level. I could not find out whether its feet were covered by the earth. It is exactly like the other figure with folded hands over the belly, aproned, and ornately tasselled on its left. It has armlets, and a ruff-like ornament round its neck. The interesting part is its head, which had been knocked off and only insecurely replaced. The thick-lipped broad-nosed face is negroid in type. The ears are large. A long lock or tassel of hair, missing on the right, hangs down from its left ear. Its nose is damaged, but there are still traces of a wale from right to left. The hair is represented by little iron pegs, inserted in small holes.' Other descriptions add nothing to this. Frobenius gives drawings of the figure (Vol. I, p. 297). Talbot gives a poor photograph (Vol. II, fig. 85) and Hambly gives a better photograph but calls the figure Olofe-finra (Plate CLVI). Bertho and Mauny illustrate it in figs. 8 and 9. These four photographs show the offering box in the same place. The huts over the figures are built at the annual festival and are sometimes only nominal.

The head of Idena figured in Frobenius's misfortunes in Ife. It was brought to him, and he bought it, but he returned it later when the Oni asked for it. Subsequently it disappeared and later turned up among his loads where he believed his interpreter had put it with the intention of selling it to him later. Frobenius had a copy of the head made by Arriens. This is now in the British Museum,<sup>8</sup> but it is a poor imitation (Frobenius, Vol. I, pp. 90f., 103, 119f.).

In the same clearing are other carvings. 'One is well-preserved, obviously a copy of an elephant's tusk, springing from a four-sided, much rougher, block' (Frobenius, Vol. I, p. 298, and fig. 2, p. 299). Dennett too calls it an elephant's tusk, two feet six inches in height 'rising from a

mound of stones' (p. 22), and Talbot 'a phallic monolith, representing an elephant's tusk and going by the name of Ikere, a "door-bolt" said to fasten Awre's door' (Vol. II, p. 340, and fig. 86). Frobenius calls it 'Idena's Staff,' but Olopo denied this and called it Ikere'Ekun and said that it was part of a gate that used to stand between two *peregum* trees and that was broken down during the Oyo wars (see fig. 2). Bertho and Mauny call it *Okpa ti Ore*, the staff



FIG. 2. POINTED STONE AT THE IDENA SHRINE

See also Plate L, background

of Ore (p. 100). In front of this stone 'are the pieces of a long narrow stone slab, fairly thin and of rough workmanship. It lies prone broken in four pieces. There is evidence of six or more holes having been bored into it at equal distances in its longitudinal axis' (Frobenius, Vol. I, p. 300, drawing on p. 299). Dennett says: 'Two were flat (one of which was broken) and measured four feet six inches by one foot, and three feet three inches by one foot respectively' (p. 22). Bertho and Mauny call it *Asa*, a shield (p. 100). These stones still exist together with some other pieces round the Idena: an open box with a lizard carved on two sides of it and a stone stool, and some other bits half buried in the ground. Talbot mentions the box, which is shown in his photograph (Vol. II, fig. 85) and Frobenius's drawing (Vol. I, p. 297).

Beyond, there is another clearing which appears once to have been surrounded by a mud wall and to have contained a hut. This is the grove of Ore. 'A decent group of all



kinds of well-preserved relics is met with in a carelessly constructed hut at the fourth and last Idena enclosure. Symmetrically placed there is a stone crocodile to right and left in front of a stone block, artificially rounded, set on end' (Frobenius, Vol. I, p. 300). Frobenius compares these blocks with two which he found in Modakeke (the Oyo quarter of Ife), with the iron 'drop' in the palace, and with a similar type found south of Timbuktu. An Ife man said that they were representations of the hammer of Ogunlade in the shrine in the palace. 'Before each of these "drop-stones" the more oval of which is twenty-four and the more conical one nineteen and a quarter inches high, there is a crocodile. The larger and better finished of the two is twenty-four and the other twenty-one inches long' (Frobenius, Vol. I, p. 300). Dennett was told that worshippers cooked and ate in the hut. 'The clerk told me that this hut once contained a slab of rock upon which a crocodile was carved. We had a good look for it, but it was not there' (p. 22). Talbot says: 'Among other examples of stone carving seen in the same grove are a couple of eggs about two feet long' (Vol. II, p. 340), and he gives a photograph of one of these and the two crocodiles (fig. 88). Frobenius gives drawings of the crocodiles and of the interior of the hut (p. 301). The tail of one crocodile has since been broken, one 'dropstone' has disappeared, as has an object shown in the drawing that looks like a large manilla. Among the fragments here are a piece of a terracotta foot (see fig. 3), and two flat pieces of iron under an upturned pot,<sup>9</sup> part of what might be a seated dog<sup>10</sup> and a piece of a base with a chevron pattern.<sup>11</sup>

The late Rev. Adejumo of Ife said that many terracottas and stone carvings which he saw before 1907 have

since disappeared. A figure on horseback (of Ore?), of a laughing man (whom Mr. Awojodu of Ife called Olofe-funra), of Alafere (the owner of the wind), and of Rere the wife of Ore are said to have gone. Obalayan, however, said that Alafere was invisible and that his image had never been in the Ore Grove.

Ore was a hunter, and was on earth before Odua (Odudua), the creator of the world, came. (When asked how Ore could have been on earth when it was all water, the priests of Orishanla said that he must have lived on an island.) He had no wife or children. Odua, however, had many children who passed Ore's home every day, so that he became jealous and began to shoot at them. One day he shot Odua's son.

Odua made enquiries and called Ore, who explained his grievance. Odua then provided him with a wife.

Bertho and Mauny describe and illustrate (pp. 103f. and fig. 11) a female figure as from the same grove. They each saw it separately in 1938 and 1949 respectively, and they record its name as Alafere. This figure was not described by previous visitors to the grove and is one which we (the present writers) have seen in the grove of Obasin further out of Ife on the other side of the Ifewara Road. Obasin, the daughter of Oremfe, led the attack which repulsed the Igbo. The figure had perhaps been brought into the Ore Grove temporarily, although the two cults are probably unrelated. Its style is quite out of keeping with the other figures in the grove, and it is probably much more recent.

Whilst working with the Nigerian Antiquities Service in Ife on special leave of absence from the Manchester Museum, I (F.W.) was asked to put back the figure of Idena in the Ore Grove. It had been removed to the Museum of Ife Antiquities by Mr. Bernard Fagg (then Government Archaeologist) in order to fit a metal dowel to hold the head in its proper position (it had previously been cemented in the wrong place). The right foot of the figure is broken, and the figure does not stand well, so a reinforced concrete slab 18 inches square and two inches thick was prepared, with impressions of the feet of the figure. It was intended to bury the slab in the ground and to cover it very thinly so that the whole figure could be seen (cf. Frobenius, Vol. I, p. 298: 'I could not find out whether its feet were covered by the earth.').

Accordingly, in March, 1957, accompanied by Mr. David Bivar of the Nigerian Antiquities Service, I set about replacing the figure. A cutting just large enough for the concrete plinth was dug, revealing a smooth piece of granite about an inch below the surface. The cutting had to be enlarged to remove the stone to allow the figure to stand upright. The enlarged trench measured four feet by three feet and was eight inches deep. The ground was a rich vegetable mould, with no signs of differentiation in the small depth excavated. When the apparently featureless piece of granite was raised and turned over, it turned out to have been carved and to have iron eyes and nostrils (see fig. 4).

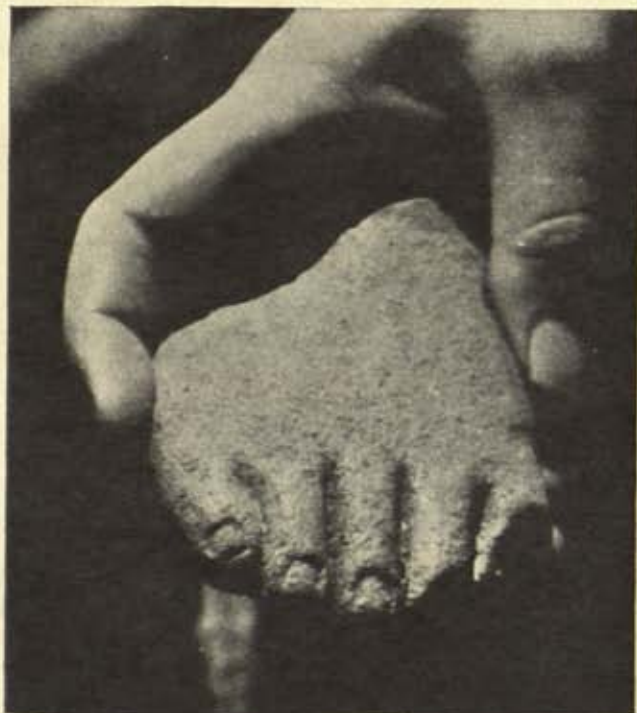


FIG. 3. LEFT FOOT FROM A TERRA-COTTA FIGURE



It probably represents a snake, or possibly a fish. There is a slight ridge on the back of the head, only visible in a raking light. The firm mouth is represented by an engraved line, and there are engraved lines along the sides of the body. The dexter side of the jaw is broken away. The dexter side of the body is straight, with the sinister side curving to meet it. The whole figure is three feet two inches long,  $8\frac{5}{8}$  inches wide and  $6\frac{3}{4}$  inches thick. The iron nails which form the eyes are  $\frac{5}{8}$  to  $1\frac{1}{8}$  of an inch in diameter and stand  $\frac{5}{16}$  of an inch above the surface of the stone. The nostrils are  $\frac{5}{16}$  to  $\frac{3}{8}$  of an inch in diameter, and project a quarter of an inch. This use of iron nails in stone is



FIG. 4. STONE SNAKE OR FISH FROM THE IDENA SHRINE

*V-shaped 'scales' are faintly discernible on the back*

closely paralleled on the Opa Oranyan in Ife, as well as, less closely, on the Idena statue itself. In contrast to the simple vigour of the head, the rear end of the figure is crude and irregular. There is no certain sign of working, which suggests that the rear end may have been buried in a bank of earth from which the animal would appear to emerge. If this were so the animal is more likely to be a snake than a fish, and snakes are represented twice on other antiquities in this grove. The fact that it lay upside-down and at right angles to the axis of the shrine suggests that it had been overturned and possibly deliberately buried.

Close to this figure and lying on the same level was half of a granite lid, with the carved top upwards. Six inches

away and two inches higher, apparently having become buried later, was the other half, lying upside-down. A small piece is still missing from the line of junction of the two pieces. They constitute the lid of the granite offering box which has been visible on the shrine for a long time (cf. Talbot, Vol. II, fig. 85, and Frobenius, Vol. I, p. 297). The lid measures  $14\frac{1}{2}$  by  $5\frac{1}{2}$  inches. It has a maximum thickness of  $3\frac{3}{8}$  inches, and bears a relief carving of a snake swallowing an expressively faced frog or lizard. The carving is up to  $1\frac{1}{8}$  inches in relief. The lower side has a rectangular cavity  $12\frac{7}{8}$  by four inches, with the junction of the sides with the bottom of the cavity well rounded. The lid is similar to the one which lies, also broken, in front of the Ore figure. Both bear the same motif.<sup>12</sup>

Near to the second piece of the lid, and presumably having become buried at the same time, was a cylinder of granite approximately five inches in diameter, with a maximum height of five inches. The top and bottom surfaces are unworked fractures suggesting that more of this may yet be found. The top of the object was already on the shrine. The two pieces together form a slightly tapering column 11 inches high, with a double flange round the ovoid top. Close to the buried fragment lay the broken flat base of a common Ife type of offering pot of uncertain antiquity. The diameter of the base is  $1\frac{1}{4}$  inches. It is crudely built with the inside unsmoothed, as is usual with these pots.

On the same level was found a bun-shaped piece of granite  $4\frac{3}{4}$  by four inches, by  $2\frac{1}{8}$  inches thick. The rounded top has a number of flats worn on it, but the bottom surface is a stepped line of cleavage. Its use is quite uncertain.

After removal of these pieces, the cutting was filled in, the concrete base suitably covered, and Idena reinstated. The new finds were placed on the shrine to be photographed with pieces already known (Plate L) before removal to the museum for cleaning. Previously undescribed pieces on the shrine are the upper stone of a saddle quern, and the stone stool, of apparently debased type, in which the handle is nowhere cut free from the stem of the stool. The existing photographs and drawings show the various items on the shrines to have been moved from time to time. In recent years no importance has been attached to the exact placing of the cult objects, but this casualness may be a recent development.

It is evident that much remains to be discovered in the shrines of Ife when so much has been found in such a very small area. It is likely that some pieces, recorded by earlier writers but no longer visible, will be found to be buried in the accumulation of leaf mould which forms continuously in these groves set in the deciduous forest.

The groves of Ife present a number of problems, some of which are most acute in relation to the Ore Grove. Most conspicuous is the general confusion of the accounts given by informants in Ife. According to Olopo one figure is of Idena and the other of Olofefunra, the servant of Ore. One wonders why the grove is named after Ore if he is not represented there. On the other hand Hambly called the grove after Idena, but the Idena figure he called Olofefunra. The figures may have been made to illustrate



existing legends, or the legends might have been evoked by the stone figures which had lost their original meaning. The confusion of the accounts of other groves, of historical traditions and of legends suggests a discontinuity of tradition. The disturbed period of internecine warfare which preceded the British pacification might have caused the break, but it is possible that the discontinuity was earlier. Some of the traditional accounts of the stone figures—e.g. that they are people who were turned to stone—remind one of European legends about prehistoric monuments, erected before the modern population of the district was constituted, and of the nature of which they are ignorant. The stone carvings are in a different style from the bronzes and terra-cottas of Ife. This may be due to the difference in material used but is more likely to be due to difference in age.<sup>13</sup> Perhaps the early Yoruba found them already there when they arrived, in which case they would be the work of the Igbo whom the Yoruba drove out of Ife,<sup>14</sup> or they might be the work of an early wave of Yoruba.<sup>15</sup>

Many of the figures in the Ore and other Groves are broken. This could be from wear and tear due especially to annual exhumation and reburial, deliberate iconoclasm, or accident in war. Talbot's story mentioned above implies a deliberate assault on one figure, though it is by no means certain that the hole could have been caused by a bullet. It seems unlikely that other Yoruba would deliberately damage the Ife shrines, even in war, since they are the ancestral shrines of all Yoruba. Many of the groves contain small fragments of antiquities which appear to have been brought there as fragments (e.g. Osangangan Obamakin<sup>16</sup> and Olokun Walode<sup>17</sup>). These could have been rescued after damage, or found as ancient remains, not understood and therefore revered. If the terra-cotta figures were removed from the shrines during the evacuation of Ife to Ifetedo during the Ibadan wars, they might have been broken in Ifetedo or on the journey in either direction. There is a further factor involved, for most of the shrines at present are sadly neglected between annual festivals, and even then damage to buildings is often not

repaired. It is possible that this neglect is traditional, but has become more serious as paganism slowly loses its adherents. It can hardly be original since those who had the figures made would surely have cared for them.

So little is as yet certainly known of Yoruba history as it affects Ife, that for some time to come archaeological excavation is likely to raise more problems than it is able to answer.

#### Notes

<sup>1</sup> The first part of this article is based on the list of Ife Shrines and Antiquities compiled by K. C. Murray, and referred to in the *Annual Report of the Antiquities Section for the Year 1946*, Lagos, p. 2.

<sup>2</sup> R. E. Dennett, *Nigerian Studies*, London, 1910.

<sup>3</sup> Leo Frobenius, *The Voice of Africa*, 2 vols., London, 1913.

<sup>4</sup> P. A. Talbot, *The Peoples of Southern Nigeria*, 4 vols., London, 1926.

<sup>5</sup> W. D. Hambly, *Culture Areas of Nigeria*, Field Museum of Natural History, Chicago, Publications, Anthropological Series, Vol. XXI, No. 3, 1935.

<sup>6</sup> J. Bertho and R. Mauny, *Archéologie du Pays Yoruba et du Bas-Niger, Notes Africaines*, Dakar, No. 56, Oct. 1952, pp. 97-115.

<sup>7</sup> Mr. W. B. Fagg suggests that the figure is the well executed representation of an achondroplastic dwarf.

<sup>8</sup> Where Mr. W. B. Fagg kindly drew my attention to it.—F.W.

<sup>9</sup> When revisiting this shrine in January, 1958, I was unable to find the terra-cotta foot, or the two iron objects which had appeared to be recent. F.W.

<sup>10</sup> This piece had disappeared by the time I first visited the shrine in October, 1956. F.W.

<sup>11</sup> To prevent the disappearance of this piece too, I took it to the Ife Museum in January, 1958. F.W.

<sup>12</sup> Talbot, *op. cit.*, Vol. I, p. 276, mentions 'two Kola boxes in stone, each one foot by half a foot' at the base of the Opa Oranyan.

<sup>13</sup> This does not imply that all Ife stone carvings are of one age.

<sup>14</sup> See H. U. Beier, *Odu: Journal of Yoruba and Related Studies*, No. 3, pp. 28f., and Rev. M. J. Walsh, *African Affairs*, Vol. XLVII (1948), pp. 231-8. One wonders whether there is any philological connexion between this word Igbo and Igbo meaning a grove in the bush. The tonal pattern distinguishes them nowadays.

<sup>15</sup> See S. O. Biobaku, *The Origin of the Yoruba*, The Lugard Lectures, 1955, Lagos, *passim*, and in *Odu*, No. 6, pp. 24f.

<sup>16</sup> *Annual Report of the Antiquities Service for the Year 1952-53*, Lagos, p. 8.

<sup>17</sup> *Ibid.*, p. 9, and *ibid.*, 1953-54, p. 13.

## THE OCCURRENCE OF THE TRAIT FOR HÆMOGLOBIN J IN A CHINESE\*

by

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**188** The first abnormal variants of adult human hæmoglobin to be described (hæmoglobins S, C, D, E and G) had a mobility on paper electrophoresis at pH 8.6 that was slower than that of normal adult hæmoglobin (hæmoglobin A). Four other variants were subsequently discovered (hæmoglobins H, I, J and K) which

\* With a table

exhibited a faster mobility at an alkaline reaction than hæmoglobin A. The latest variant to be described (hæmoglobin L, Ager and Lehmann, 1957a) is also slow-moving.

Hæmoglobin J was first described in a Negro family (Thorup *et al.*, 1956) and has since been detected in two members of an Indonesian family of Javanese extraction (Huisman *et al.*, 1957), in three members of an Indonesian



family from Minahasa, the northern part of Sulawesi (Celebes) (Lie-Injo, 1957), in a Caucasian of French Canadian ancestry (Col. W. H. Crosby, quoted by Robinson *et al.*, 1956), in a Gujarati Indian living in Uganda (Raper, 1957) and in two other Gujarati Indians living in Bombay (Sanghvi, Sukumaran and Lehmann, 1957). It has also been detected by one of us (F.V.) in a normal healthy adult Punjabi Sikh—a blood-donor—who came to Singapore from Ludhiana district, Punjab, a few years ago. Four members of an Indian family which originated from Mayavaram, Madras, India, and now resides in Singapore have also been found to be carrying the haemoglobin J trait (Vella and Wells, 1957).

The only fast-moving abnormal haemoglobin recorded in Chinese so far has been haemoglobin H. Of all abnormal haemoglobins this is the fastest-moving. It is always associated with a disease state, denatures rapidly on freezing, is associated with the presence of intra-erythrocytic inclusion bodies on supra-vital staining of erythrocytes containing it with Brilliant Cresyl Blue and is unique in exhibiting an anodal mobility at both slightly acid and alkaline reactions in virtue of its low isoelectric point ( $pH$  5.6). Haemoglobin H was originally described in two Chinese siblings in America (Rigas *et al.*, 1955 and 1956), but has since been found in Siamese (NaNakorn and Minnich, 1956), in Indonesian Chinese (Lie-Injo, 1956), in Chinese in Singapore (Vella, 1957), in a Nepalese Gurkha woman (Brain and Vella, 1957) and in a Malay (Lehmann and Singh, 1957). Non-mongoloids found to be carrying the haemoglobin H trait include some Greek families (Gouttas *et al.*, 1955), a Transjordanian Arab (White *et al.*, 1956) and a Greek Cypriot (Ager, quoted by Ager and Lehmann, 1957b).

#### Method

In a survey of the incidence of abnormal haemoglobins in Singapore it became possible for one of us (F.V.) to investigate a number of clotted blood samples after the blood serum had been used for Wassermann and Kahn Tests at the General Hospital, Singapore. The haemoglobin was extracted from the clots by shaking manually with a small amount of distilled water and separating the haemoglobin solution from the broken fragments of clot. The solution was then treated with toluene, and a clear preparation of haemoglobin was pipetted out from under the solid toluene layer which separated out after centrifuging for 15 minutes at 2,000 r.p.m. On study by the technique of paper electrophoresis with the paper kept horizontal and sandwiched between silicone-treated glass plates (Chernoff, 1955), one of these samples showed a fast-moving component (about 30 per cent. of the total) in addition to normal haemoglobin. The fast component had the properties described for haemoglobin J. Fresh blood samples from the patient exhibited this fast fraction on every occasion that they were studied. A sample of blood was studied for us by Dr. H. Lehmann of St. Bartholomew's Hospital, London, and found to be identical with a standard sample of

haemoglobin J and different from haemoglobins H, I and K.

#### The Patient

The patient is a Teochew Chinese male, aged 46 years, who was under remand as an opium addict. He is a healthy-looking individual of medium build. His only complaint was that he had poor vision in his left eye. He was an only child of parents who were both born in Haiyeow, 15 miles west of Swatow. His mother is still alive, aged 66 years. She has been blind for the last 20 years but is otherwise well. His father died at the age of 31 years. The patient came to Singapore from China some 30 years ago and has an only son who is living in China, aged 21 years. No family history of anaemia was elicitable and he gave no history of previous illness.

On examination he was found to have bilateral ptosis. His left pupil was smaller than the right and there was an early cataract in the left eye. His liver was palpable one inch below the right costal margin and was firm, smooth and not tender. The spleen was not palpable. There were dilated cutaneous venules around the right costal margin. No other abnormal physical signs were found. X-rays of his chest, skull and hands were normal. The findings in his blood were as follows (in brackets, normal range for adult male Chinese).

Haemoglobin: 13.9 grams per 100 ml. (13.5–16)  
 Erythrocytes: 4.9 million per cubic mm. (4.5–5.5 million)  
 Reticulocytes: less than 0.5 per cent. (0–1)  
 Packed Cell Volume: 43 per cent. (40–50)  
 Mean Corpuscular Volume: 88 cubic  $\mu$  (78–94)  
 Mean Corpuscular Haemoglobin: 28  $\gamma\gamma$  (27–32)  
 Mean Corpuscular Haemoglobin Concentration: 32 per cent. (32–38)  
 Mean Corpuscular Diameter: 7.3  $\mu$  (Halometry) (7.2–7.8)  
 Leucocytes: 7,000 per cubic mm. (5,000–10,000)  
 [Neutrophils: 78 per cent. (60–75)  
 Lymphocytes: 19 per cent. (15–30)  
 Monocytes: 3 per cent. (2–10)]  
 Serum Bilirubin: 0.2 mg. per 100 ml. (less than 0.8)  
 Alkali-Resistant Haemoglobin: 1 per cent. (0–2)  
 Wassermann and Kahn Tests: Negative.

Stained films of his peripheral blood showed no abnormality. No intra-erythrocytic inclusion bodies could be demonstrated on supra-vital staining with Brilliant Cresyl Blue. Sickling of the erythrocytes could not be elicited by admixture of the blood with sodium metabisulphite solution (2 per cent.).

#### Comment

In contradistinction to the majority of the slow-moving haemoglobins, the fast-moving haemoglobins have been described only in sporadic cases or single families. Table I shows the incidence of the trait for haemoglobin J as found by those who have so far reported cases and in the Singapore survey. The homozygous state for this abnormal haemoglobin has not yet been described.

None of the findings in the blood-examination are in any way abnormal and the presence of the gene for haemoglobin J in the heterozygous state appears to be in no way



deleterious. It is unfortunate that no other member of this patient's family was available for study.

TABLE I. INCIDENCE OF HÆMOGLOBIN 'J' TRAIT

Author	Ethnic Group	Cases of Hgb. J Trait	Number Studied	Remarks
Thorup et al. (1956)	Negro	7	13	All in one family
Crosby quoted by Robinson et al. (1956)	French Canadian	1	Not given	
Huisman et al. (1957)	Javanese	2	Not given	Both in one family
Lie-Injo (1957)	Indonesian	3	390	Three in one family. From Minahasa, Celebes
	Javanese	nil	500	
	Indonesian	nil	6000	From other regions
Raper (1957)	Indian	1	326	In Uganda
Sanghvi et al. (1957)	Indian	2		In India
Singapore Survey	Indian	5	1020	One Sikh. Other four in one family
	Chinese	1	3400	Present case
	Malay	nil	2100	
	European			
	'Whites'	nil	1960	
	Nepalese	nil	470	
	'Eurasians'	nil	400	

One other instance of the occurrence of an abnormal hæmoglobin previously unrecorded in Chinese was the case of a 24-year-old male of Hokien ancestry whose family originated in Amoy and who, during investigation for a Thalassæmia-like hæmolytic anæmia associated with unusual X-ray findings, was discovered to be carrying a combination of hæmoglobin H and a previously undescribed slow-moving variant in his blood. His mother, the only relative available for study, was found to be carrying the trait for the new hæmoglobin which has been named hæmoglobin Q (Vella et al., 1958).

It is to be expected that more extensive surveys will elucidate the incidence of these rarer forms of abnormal hæmoglobin and will unearth cases of homozygosity which will be of particular interest.

#### Summary

The finding of a Chinese male adult heterozygous for normal hæmoglobin and for hæmoglobin J is reported. No physical or hæmatological abnormalities were detected which could be attri-

buted to the presence of the abnormal hæmoglobin. The incidence of the hæmoglobin J trait and other abnormal hæmoglobins found in Chinese are reviewed.

#### Acknowledgments

We are very grateful to Dr. H. Lehmann of St. Bartholomew's Hospital, London, for comparing this patient's hæmoglobin with standard samples of hæmoglobins H, J and K; to Dr. Lie-Injo Luan Eng of the University of Indonesia, Djakarta, for permission to refer to her unpublished observations; to Dr. L. A. da Silva and Mr. Sabaratnam of the Pathology Department, General Hospital, Singapore, for making available to one of us (F.V.) blood samples submitted to them for serological investigations; to Dr. (Mrs.) M. M. H. Gibson-Hill, of the Blood Transfusion Service, Singapore, for making available to one of us (F.V.) several thousand blood specimens from blood-donors; and to Mr. Stephen Pang for valuable technical assistance.

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## SHORTER NOTE

### Enthusiasm and Restraint in the Study of Human Evolution.

By Dr. E. H. Ashton, Department of Anatomy, University of Birmingham

**189** The popular interest which, during the past 100 years, has attached to the study of human evolution has provided an immense stimulus to the professional student who owes much to the energy and perseverance of those amateur archæologists who have been responsible for discovering remains of fossil man. Gratitude, however, sometimes has to be tempered with embarrassment, for the amateur, overcome by enthusiasm, all too often attempts to interpret the significance of his discoveries and thereby introduces confusion into a field which even in the hands of the professional is only too open to speculation and the statement of foregone conclusions.

For the serious student, the analysis of human evolution has as its background the body of genetical and evolutionary theory accumulated during the present century, and in studies of geographical variants of living man close attention is now given not only to those anatomical characters on which the classical subdivisions were first based, but also to physiological features of known genetic or selective significance. Occasionally limited inferences can be drawn about the selective significance of bony features, but as little is known about their genetic basis, an appraisal of the evolutionary status of extinct groups still of necessity rests mainly on an assessment of similarities and differences in skeletal morphology.

Comparison between groups of interrelated features is facilitated by modern biometric and statistical techniques, and the overall



degrees of resemblance and divergence thus defined are summarized broadly by the present-day system of zoological classification. When the various fossil and extant groups which emerge are viewed in relation to their geographical distribution and geological age, there can be built up a three-dimensional view of human diversity. The picture becomes more complete as additional material is discovered, but however extensive the available remains may become, no discussion of anatomical or genetical matters can decide whether, on the one hand, an established degree of difference is too great to preclude the possibility of a direct relationship or whether, on the other, it is small enough to make such a connexion probable.

Similar limitations attach to the inferences that can be drawn from a study of fossil and extant apes, and although the material now available provides a relatively extensive picture of variation within the group extending from the Oligocene to the present day, little can be deduced either about the anatomical nature of the ancestors of the extant apes, or about the time at which the human and ape stocks first diverged.

As a result, a generally restrained tone has now come to characterize most accounts of man's ancestry, but, occasionally, speculative writings, resembling in some respects the popular accounts of an earlier era, still originate from those whose enthusiasm has led to a demand for a more complete picture of human evolution than can be provided by modern taxonomic practice. For example, Professor L. S. Palmer's recent book, *Man's Journey through Time*,<sup>1</sup> written by one who, although for many years interested in study of fossil man, has as a profession 'studied and taught Physics,' attempts an ambitious integration of some aspects of anthropology and geochronology to form the new discipline of 'Anthropochronology.' The work is, in effect, an introduction to quantitative methods of studying human evolution and it is greatly to be regretted that throughout the sections dealing with man's physical development the text shows a complete departure from the standards of biological and anatomical criticism that have come to characterize modern evolutionary studies.

The stated purpose of Professor Palmer's work is to depict man's evolutionary development 'in a graphical and quantitative manner,' the approach being to plot against the geological age of each type of fossil man and ape, first a series of anatomical indices which the author believes 'vary most conspicuously with increasing hominid characteristics,' and secondly a series of 'criteria of culture.'

The derivation of the time scale is discussed part way through the volume in a section which is virtually a summary of the extensive reviews of the field published in recent years by Professor F. E. Zeuner.

Man's physical development is summarized by analyses of presumed secular changes in four cranial features. The first is the height to which the nuchal muscles extend relative to the Frankfurt plane and is expressed as the 'nuchal area height index.' The second is the position of the occipital condyles relative to the anterior and posterior extremities of the skull and is expressed as the 'condylar position index.' Both these quantities were used some eight years ago by Professor Sir Wilfred Le Gros Clark in a study of the South African Australopithecinae, and were believed by him to be related to posture. This view is accepted uncritically by Professor Palmer regardless of the fact that further analyses have subsequently shown that the correlation between these indices and posture is by no means close enough to allow inferences to be made about variations in the gait of fossil types. The third quantity used by Professor Palmer as a criterion of humanity is a simple assessment of the cranial capacity, while the fourth measures the convergence of the lower dental arcade—a feature supposedly associated with the capacity for speech-formation.

The graphs derived from the four anatomical indices purport to show a steady change with time leading from a stage represented by *Pithecanthropus* through groups represented by the skulls from Steinheim and Skhul towards *Homo sapiens*. Apart from the curve relating to the angle of convergence of the lower dental arcade, these graphs appear to flatten out as they approach the men of the Upper Palaeolithic and present-day Europeans. A second line branches off from the first some 250,000 years ago and leads through Neanderthal man on to the living Australian Aboriginal or, in the case of the 'condylar position index,' towards the chimpanzee. Although the author is careful to point out that he does not wish to imply 'a genetic relationship between the several skeletons' and that the graphs are merely 'time sequences of certain anatomical indices,' the very nature of the presentation implies a transition from stages represented by various archaic men to the types of the present day. This impression is considerably reinforced when Professor Palmer enthusiastically notes that extrapolations of the graphs which pass 'close to the index values for *Proconsul africanus*,' while not necessarily proving 'that man has ascended (or descended) from this primitive Miocene ape,' show that such a relationship is possible. It is, to say the least, unfortunate that the author even hints at the possibility of direct relationship as it is on the validity of this type of assumption that modern systematic studies have thrown probably the most doubt. Even the most uncritical reader can, however, have little doubt as to Professor Palmer's real views on these matters when he finds that the reason for including on the graphs the various indices for a chimpanzee rather than those for other apes is that 'most authorities agree that the chimpanzee has closer affinities to European man than have either the gorilla, the orang-utan or the gibbon,' and when he finds that this is followed by corresponding speculations about the orang-utan and the Mongolian on the one hand and about the gorilla and the Negro on the other. With the text characterized by major shortcomings of this type it would seem almost pointless to list the numerous other faults that are evident in Professor Palmer's treatment of the data, except perhaps to note that his disregard for modern systematic practice is coupled with a complete failure to take account of the variation existing in different characters at each time level—this observation applying even to those cases such as the Modern European, the Australian Aboriginal and the chimpanzee for which much of the relevant information has already been published.

The difficulties involved in attempting to quantify states of cultural development are well known, and in his discussion of suitable criteria Professor Palmer shows somewhat more caution than is evident in other sections of his work. His final selection comprises first the numbers of materials used by man, secondly the number of occupations in which he was engaged and thirdly the speed with which he was able to move by mechanical means.

The major criticisms of the treatment of the anatomical data apply with equal force to the graphs depicting man's cultural evolution. Here, however, the final sections which relate exclusively to the cultural development of *Homo sapiens* show an enormous upward surge as the present day is approached. The onset of this technological advance is estimated to have been during the Upper Palaeolithic and is attributed to the development of speech and the use of symbols.

The concluding sections of this book comprise speculations about man's future evolution. Although Professor Palmer points out that the present rate of cultural development will almost certainly change in the foreseeable future, he does not hesitate to extrapolate the graphs depicting physical development and by virtue of three out of the four curves having already flattened out, claims that 'there is no reason to expect any marked changes for the next few thousands of years, except possibly in the speech



faculty.' This last phase derives from the fact that the curve depicting changes in the angle of convergence of the mandibular rami appears, in contrast to its fellows, to be still rising at the present time. Even if attention is, for the moment, diverted from all other shortcomings in this book, the fallacies inherent in any attempts to use curves of this type to predict future developments cannot fail to be immediately evident to all who are conversant with the wider aspects of vertebrate evolution. The danger inherent in Professor Palmer's work is that its ambitious scope and attractive presentation will appeal immediately to those who

have not yet had an opportunity to appreciate the basic anatomical and biological principles involved, and who may in consequence be led to a completely false appreciation both of what is already known of man's evolutionary history and of the problems that derive from the limitations in present-day knowledge of the subject.

#### Note

<sup>1</sup> London (Hutchinson), 1957. Pp. xvi, 184. Price £1 10s.

## CORRESPONDENCE

### The Prehistory of China. Cf. MAN, 1957, 222

**190** SIR,—In his review of Kenneth Starr's English version of Li Chi's and his collaborators' report on the excavations at Ch'eng-tzu-yai, Mr. Watson writes: 'The motive for the publication of this translation is possibly the very laudable one of making available an all but unobtainable book. There can hardly be another, since anyone prepared to read so detailed a report must be a specialist, who again could hardly be such unless he were able to read excavation reports in Chinese.'

This is a most regrettable statement which should not pass unchallenged. Is Chinese archaeology to be an *arcana* which only professional sinologists are allowed to approach? Are there no relations at all between the prehistoric cultures of China and those of the rest of the Old World, and is China to remain a blank on the archaeological map of Asia for all those who do not read Chinese? If we were to adopt Mr. Watson's view, this would mean sanctioning that kind of narrow specialism which in the past has wrought so much harm. In the present case it would be the more harmful since very few sinologists possess the training and knowledge which would enable them correctly to assess and interpret the meaning of prehistoric finds. This should not be understood as a reproach. Sinology is such an exacting subject that it leaves its adepts little time to acquire equal mastery in other fields.

I can assure Mr. Watson that Starr's translation was enthusiastically welcomed by myself and other archaeologists. We urgently need many more translations of this kind. At the very same time when Mr. Watson's review was published, in November, 1957, I proposed at a U.N.E.S.C.O. symposium in Tokyo a resolution 'that U.N.E.S.C.O. approach the International Council for Philosophy and Humanistic Studies in order to undertake the study of the possibility of establishing an international organization for the translation of important scholarly works in the humanities and social sciences.' This meant, of course, translation of modern Japanese and Chinese works on archaeology, anthropology, history, etc., into English or other western languages. The resolution and another somewhat similar one proposed by the Japanese historian, Professor Tatsuro Yamamoto, were unanimously adopted.

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### Joking Relationships in Africa. Cf. MAN, 1957, 140, 225; 1958, 21

**191** SIR,—May I refer to some points in Mr. V. Reynolds's letter?

(1) It is not to be doubted that various forms of intra-tribal *utani* or joking relationships occurred in some Tanganyika tribes in pre-European times, and Mr. Reynolds's example relating to Zaramo clans is a satisfactory enough case in point. Inter-tribal *utani* is another matter, and unfortunately Mr. Reynolds's example of the Bondei is less happy. Quoting Moreau (*Africa*, Vol. XIV, 1943, pp. 386-400) Mr. Reynolds writes that 'some Bondei recognize rights of mutual hospitality, which are perhaps vestigial, with certain of the Digo.' But the Bondei as an independent unit or

tribe seem to be of no great antiquity. Genealogies which I have recorded from them in their own country show that families who now call themselves Bondei have acknowledged origins, in the generation of the grandfather or great-grandfather of present-day old men, amongst the Zigua or Sambaa or Digo; and Bondei clans are said to have originated from clans with the same names in these tribes. It is, therefore, not surprising that some Bondei have, or had earlier, *utani* relationships with some Digo. Probably, though I do not know certainly, other Bondei have or had *utani* with some Zigua or some Sambaa and for the same reasons. Thus *utani* between Bondei and Digo scarcely seems to demonstrate the existence of long-standing inter-tribal *utani*. Of course, it does not disprove it either, but it does indicate that care is needed in the use of information on this matter.

(2) The likely fact that *utani* grew up between the Zaramo and the Nyamwezi as a result of the early migration of the Nyamwezi as porters along the Central Tanganyika caravan routes to the Coast does not seem to conflict with the hypothesis put forward by Professor Wilson concerning the Nyakyusa, and supported by my Ngoni evidence. This Nyamwezi movement was a form of labour migration just as much as the later movement to European estates: both were the direct result of external agencies (Arab, Swahili and European) which provided new sources of employment and the peaceful conditions under which they could be taken up.

In parenthesis, is it necessary nowadays to speak of the so-called 'wanderlust' of a tribe or people as an explanation of labour migration? Our considerable knowledge on this matter from both East and Central Africa should preclude the use of this old and vague blanket term.

(3) It is perhaps a little ungracious on the part of Mr. Reynolds to suggest that Professor Wilson and I have each been misled by our informants who have so dressed up their accounts of the origins of their *utani* that we have failed to perceive the truth and have missed pointers to the existence of pre-European inter-tribal *utani*. I think that I can assure him that the Ngoni did not have *utani* with the Nyakyusa whom they raided and fought intermittently and from whom they were separated by at least some 120 miles of difficult highland terrain. I am quite certain that the Songea Ngoni did not have *utani* with the Sukuma or Nyamwezi in pre-European times for the simple reason that the latter's territories are hundreds of miles distant and no contact was made between members of these unconnected tribes until labour migration began in early German days. As I said in my earlier letter, the Songea Ngoni did not have *utani* relationships with other tribes: they only raided and exploited them.

Finally, although I described the Ngoni *utani* relationships with other tribes as a comparatively recent phenomenon, it must be remembered that, like the Nyamwezi, the Songea Ngoni began working as porters in caravans to the Coast at Dar es Salaam, Lindi and Mikindani in the first decade of this century and they were amongst the very first to migrate to work on the new plantations and estates at or near the Coast. Thus the need for *utani* and, I think, the relationships themselves between these Ngoni and certain other tribes go back now for almost half a century.

Arusha, Tanganyika

P. H. GULLIVER



## REVIEWS

## GENERAL

**The Corridors of Time.** By H. Peake and H. J. Fleure. Oxford (Clarendon Press) (London: Cumberlege), 1927-56. 10 Vols. Price: Vols. I-IX, 15s. each, Vol. X, £2 2s.

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Since it began to appear in 1927, *The Corridors of Time* has been for untold students an unfailing friend and a sure guide along the devious and intercrossing paths of anthropology, prehistory and comparative stratigraphy. The appearance of the tenth and concluding volume is an appropriate occasion for inquiring how the whole work has weathered with time.

The last volume, which opens sadly with an obituary notice of Peake, is in effect a synopsis of the preceding nine. It also adds some necessarily brief observations which bring the story from the Iron Age of Vol. IX up to the present day. But after the first three chapters on the early stages of the Evolution of Man, the authors have abandoned the strict chronological arrangement characteristic of the earlier volumes, and they describe the cultural evolution of the Old World, from neolithic times onwards, region by region. In addition to being arranged differently from its predecessors, Vol. X supplements them on certain points, for instance by giving a fuller description of the Levallois flaking technique, and it also, of course, takes advantage of more recent discoveries. It makes no attempt, however, at a methodical revision of the earlier volumes. Thus, while it naturally omits Piltdown Man, there is no recantation of the paragraphs concerning him in Vol. I.

On reading Vols. I-IX, which appeared in the decade 1927-36, it is surprising to see how little of importance calls for definite alteration, doubtless because the views originally expressed, though by no means always orthodox, were most soundly based, and often showed remarkable foresight. For instance, an opinion of 1927 on the possible origins of man in Africa (Vol. I, pp. 58f.), though not popular at the time, has been well supported by subsequent finds; though it is strange that *Proconsul* is not mentioned in Vol. X, and the importance in this context of the Australopithecines seems, in view of doubts about their tools, to be too heavily stressed.

In the study of races generally, Vol. X, like Vol. III, Chapter 9, places great reliance on skull form, and treats dolichocephaly in conjunction with heavy eyebrow ridges as a primitive character. With special reference to Vol. X, p. 64, the question may be asked, whether skull form is indeed less prone than other racial features to environmental selection. The study of the influence of climate on racial physique has thrown so much light on Mongol origins, in particular, that it is strange that this factor is not mentioned in Vol. X, p. 254, alongside the more hazardous suggestion of a *Sinanthropus* strain in the modern Chinese.

There are other topics on which even Vol. X may be thought to be unduly conservative. The conventional classification of stone implements by shape rather than by technique and function is largely retained, and in particular much is deduced from the distinction between core tools and flake tools in the Lower Palaeolithic. The practical study of the flaking and use of flint might seem to call for some revision of the old categories, and especially for serious questioning of the human origins of alloliths.

Again, in Vol. X, pp. 286f. and 291f., as in the whole of Vol. IV, great reliance is placed on the typological study of megaliths for the understanding of racial movements. Today such arguments appear rather tenuous, especially when the evidence is collected from such wide distances, as is indeed admitted in Vol. X, p. 248.

On the much neglected but crucial question of the time and circumstances of the spread and adaptation of the idea of cultivation in the remoter parts of the Old World, the authors have disappointingly little to say. It would have been valuable to know their ideas on when and where yams were first grown, on whether 'wet' rice preceded 'dry' rice cultivation, and whether the type of agriculture necessary on the oasis route to China set the pattern for the technique of rice farming. In Vol. X the authors keep to their early opinion on the uniqueness of the Neolithic of south-west Asia, and wisely ignore recent speculations about an independent discovery of cultivation in south-east Asia. But if, in fact, Chinese cultivation

is derivative, the isolation of China from the west must have been less complete than is implied in Vol. X, p. 254. Similarly, more might have been said about the way by which agriculture came to tropical Africa, especially in the light of evidence from Khartoum.

On the question of Polynesian origins, the authors try to have the best of both worlds, postulating colonization from both east and west. But the whole study of the peoples and cultures of the New World is deliberately, though regrettably, excluded from the survey.

In general the production of Vol. X is of the careful and neat standard to which the earlier volumes have accustomed us, though 'parallelism' is misspelt on p. 223, and the grotesque 'mierbliths' of the table on p. 77 should be changed in the next printing to 'micro-liths.' There are small points on which it would be possible to take issue. For instance, the distinction and range of the pre-painted monochrome pottery of neolithic south-west Asia is not sufficiently stressed in Vol. X, p. 89. Or again, with special reference to the speculations in Vol. IV, pp. 124 and 149, which are not amended in Vol. X, it is now almost certain that the Thessalian Neolithic came by sea from Syria and not overland through Asia Minor; though recent finds at Hacilar near Burdur support the conjecture in Vol. IV, p. 174, that this culture came inland from Pamphylia (Pisidia had no coast).

But it is, in fact, as irrelevant to question details in a series of this sort as it would be to complain of its flimsy bibliography. *The Corridors of Time*, like *Primitive Culture*, *The Golden Bough* and *The Dawn of History*, is a work which it would almost be sacrilege to consult on a particular issue. It belongs to an order of creation different from that of the flawless but pedestrian textbook or encyclopaedia. It has helped to define the range and method of its subject, and to prove it worthy of serious study; and generations of students will yet turn to it to see how fine intellect, with a talent for broad synthesis and clear expression, can give life and purpose to this discipline.

W. C. BRICE

**Meeting Prehistoric Man.** By G. H. R. von Koenigswald. London (Thames & Hudson), 1956. Pp. 216, 24 plates, 48 text figs. Price £1 1s.

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This is an attractively written book, but its slightly frivolous title may prevent some of those who would find it rewarding from taking the trouble to read it. The author has attempted to cover a wide range of interests and to cater both for the layman and the reader with specialized knowledge. On the whole the attempt is a successful one. The book builds up a picture of the present state of knowledge of the evolution of man, as the author sees it, by recounting the relevant discoveries at sites which the author has worked or visited.

The greatest value of this book from the anthropologist's point of view lies in the descriptions and evaluation of finds in Java. The author was intimately concerned with many of the discoveries made there between the two wars, and he was also involved in a prolonged controversy with Eugène Dubois who discovered the first skull of Java Man. The history of that discovery and of the progress of its interpretation over the last 65 years has not been told so fully before.

Dubois went to the Dutch East Indies as a physician in Government service, but he had been influenced by Darwin's writings and already had in mind the possibility of contributing to knowledge of fossil man in that part of the world. He managed to get himself commissioned to investigate caves in Sumatra in 1889. When it was reported that a human skull had been found in a travertine quarry at Wadjak in Java he got permission to switch his activities to that island, and this led to his discovery in 1891 of fossils in the ancient river beds at Trinil, including an upper molar tooth and a skull cap with strong supra-orbital ridges which Dubois attributed at first to chimpanzee. In the following dry season he found at the same level a complete femur, which he recognized as adapted to upright posture. He attributed all three specimens to the same individual, and published an account of them in 1894 under the title '*Pithecanthropus*



*erectus*, a Man-like Species of Transitional Anthropoid from Java.' (As Haeckel invented the name *Pithecanthropus* in 1863 for a hypothetical creature transitional between man and the anthropoids, its validity could be questioned under the International Rules of Zoological Nomenclature.)

When Dubois's views failed to gain general acceptance he became strangely introverted and would rarely allow anyone to see his finds, which he continually feared would be stolen.

Von Koenigswald examined the skull in 1936 and realized that until a specimen with intact ear region came to light uncertainty would remain about the status of *Pithecanthropus*, which Dubois still maintained was a giant anthropoid. Von Koenigswald resolved to revisit Java, and within one year he had discovered in the Trinil beds at Sangiran a second skull of *Pithecanthropus*, almost identical with the first but with the region of the ear hole sufficiently well preserved to leave no doubt that the skull was not anthropoid but human, although with the smallest recorded capacity (775 c.c.).

The second skull had been recovered in 40 pieces. The author says, 'too late I realized that my opportunist brown friends were breaking up the larger pieces behind my back in order to get a bigger bonus,' for he had promised to pay 10 cents for every additional piece belonging to the skull.

Dubois did not accept von Koenigswald's interpretation of the new skull, which he said had been falsely reconstructed, although in fact radiography eventually disclosed the positions of the closed sutures and proved that von Koenigswald's reconstruction was correct.

The second chapter on Java includes a full account of the discovery of human remains in the Djetis beds, which underlie the Trinil beds, and are regarded by the author of the book as of Villafranchian age. These remains are generally accepted as the oldest known remains of man, but there is still some doubt whether they are as old as the earliest remains of *Australopithecus* in South Africa. The material from the Djetis beds includes the jaws of '*Meganthropus*' and the adult skull variously named *Pithecanthropus* IV, *P. robustus* or *P. modjokertensis*. One of the most fascinating discoveries in palaeontology is that of the skull of the infant *Pithecanthropus modjokertensis* brought to light by von Koenigswald at Modjokerto in 1936. When the discoverer described it as a species of *Pithecanthropus*, Dubois protested that it could not be—because it was patently human, whereas *Pithecanthropus* was an anthropoid! (Being the skull of a 2-year-old infant it lacks the projecting brow ridges characteristic of the adult.)

The 11 human crania of Java neanderthaloids found at Ngandong in the Solo Valley are probably rightly interpreted by the author as the trophies of Pleistocene head-hunters. (Although this is not recorded in the book, one of the Solo skulls was found in the Palace of the Emperor in Tokio at the end of the war, and on being recovered by the Americans gave rise to the rumour, so difficult to kill, that the Americans had possession of the skulls of Peking Man.)

The chapter on China includes an excellent account of the history of research on Peking Man, and of the author's discovery in 1935 of 'giant' primate teeth (*Gigantopithecus*) among 'dragon's teeth' bought from Chinese apothecaries' shops. Since the publication of this book, a lower jaw of *Gigantopithecus* has been found in a cave in Kwangsi, and there now seems little doubt that this creature was an ape.

The author considers that the dental differences between Peking Man and Java Man are sufficient to justify generic separation. This is contrary to the general trend of opinion which is towards lumping rather than splitting.

In the chapter on Africa, the remains of the East African Miocene ape *Proconsul*, and of the South African Australopithecines are discussed. The latter are given the popular name of Half-Men, which would be perhaps more acceptable as a popular name in German for it is on a par with *Halbaffen* (lemurs).

Professor von Koenigswald rather naturally favours an Asiatic origin of man, and this theory may have coloured his thought in regarding the Australopithecines as 'the animal equivalent of man,' but *Meganthropus* as 'positively human.'

The African chapter includes an account of the remarkable palaeolithic discoveries in the Oldoway Gorge, Tanganyika, and at

Olorgesailie in Kenya. Certain questions about the skeleton of Oldoway Man are asked on page 166, but the reader's curiosity is left unsatisfied. In fact this skeleton was a Late Palaeolithic burial. The chapter on Europe includes a very readable account of the Piltdown forgery and of the Lascaux cave paintings.

Some of the illustrations are excellent, particularly the plates, the choice of which reveals the author's artistic outlook. The only serious defect is the lack of any indication of scale in the figures of many of the artifacts.

KENNETH OAKLEY

**Le Razze e i Popoli della Terra: Vol. IV, Oceania, America and General Index.** By Renato Biasutti. 2nd edn. Turin (Unione Tipografica-editrice Torinese), 1957. Pp. 811, 11 plates, 538 text figs. Price Lire 9000

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This is the last volume of a comprehensive account of the physical and cultural anthropology of our world. The earlier volumes dealt with general questions and with the particular aspects of what a developed ethnodynamics would call the great 'source' of mankind—Eurasia and Africa, the cradle of our kind. This part deals with the two great 'sinks'—America and the Pacific islands. The work has been shared by four other authors. Raffaello Battaglia deals with the Pacific region, a chapter on its present-day ethnology being contributed by Giuseppe Gentili. Similarly, José Imbelloni deals with the earlier Amerindians, and with South America in detail, Tullio Tentori ending the book with an account of the modern American racial melting pot. Signor Biasutti himself deals in detail with the North American continent. The emphasis is on the cultural side, but the physical aspect is given reasonable space. The reader is guided through the ocean and jungle of fact and hypothesis by excellent photographs and 38 clear maps showing the distribution of peoples, languages and cultural objects or practices. A set of beautifully coloured plates ranges in content from a Dakotan Indian, through head-hunters' trophies, totem poles and pottery, to ancient drawings and inscriptions. Each of the 16 chapters has a good bibliography.

It was a happy thought to put Oceania and America together. As has been said, they are the great sinks towards which the eastward or southbound tides of man have streamed, just as Iceland, our own islands and the Azores-Canary group were for so long the sinks of the westward flow. Prescinding from colour, which is separable genetically from size and shape, a study of the material figured in these pages raises anew the notion of three successive streams of human type—Australoid, Europoid and Mongoloid, to use the most non-committal terms to hand. It is startling to find photographs of two males and two females of seemingly pure Easter Island stock which might have been taken from any current issue of the *Cork Examiner*. Conceding (*pro argumento*) that Corkmen are European, we have in this either a demonstration of the European affinities of the Polynesians or else a remarkable instance of convergence during the internal evolution of our species. The treatment of the Polynesian problem, including the *Kon-Tiki* affair, is balanced; although the very recent claim regarding a New Zealand folk (*Waitaha*) of pre-Christian (B.C.) date poses new questions here.

The sections on the Americas form a useful introduction for anyone trying to orient himself in this rapidly expanding field of anthropology. The four volumes would make an excellent text for any undergraduate reading anthropology who has an elementary knowledge of Italian; for the language is simple and the presentation clear. The printers have done a job in the full tradition of their country.

M. A. MACCONAILL

**The Transformations of Man.** By Lewis Mumford. London (Allen & Unwin), 1957. Pp. 192. Price 15s.

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Mumford is a synthesizer in the tradition of Ibn Khaldun, Montesquieu, Guizot, Kropotkin and Geddes. He has read and thought widely and speaks frankly and personally, thus avoiding what Sir Winston Churchill has described as the unhappy fault of being always in the right. He sees the story of man as a series of transformations rather than as a continuous evolution and he notes the following as marking the great changes:



(1) The developments of Archaic Man, before cities began, (2) Civilized Man, i.e. men having cities, usually with local gods, (3) Axial Man with increase of communications and of dominion and with religions tending towards universality, (4) Old World Man with enclosed cultures and cyclic breakdowns leading to foul inquisitions, long ecclesiastical and lately political, (5) New World Man since the spread of printing, anxious to make rather than to follow precedent and to change from the subjectively conditioned to the object-centred attitude to life, (6) Post-historic Man, mechanizing wherever possible and intent, Mumford ironically suggests, on superseding the activities of the frontal lobe of the brain. He and many others think that we are witnessing and feeling the acute birth throes of world culture, in which capitalism and communism will interpenetrate, and various philosophical, religious and aesthetic traditions will have to infiltrate into one another. This is suggestive, and, on the whole, hopeful if iron and ecclesiastical curtains and other *Apartheids* can be holed.

No two thinkers would make quite the same sequence of transformations in working out man's story, even from Mumford's social and anthropological point of view. A very early transformation when hunting spread must have greatly increased differentiation between men's work and women's work and developed group activities both in hunting and around the dance place and campfire. Later on, cultivation of the soil made a transformation that men have long recognized as important—has not Mesopotamian folktale summarized this for us as the Fall of Man? Mumford might have differentiated more explicitly between the teachers who, from about 600 B.C. to about A.D. 650 promulgated the various universalist religions, the phase which our author discusses as that of Axial Man. Old World Man is seen mainly as of our Roman and Middle Ages with their ceaseless wars, persecutions and atrocities. New World Man comes after printing made the old repression of thought difficult, and men sought to escape from old miseries by new enterprise. The chapter on post-historic man shows Mumford's hatred of the dominance of machines over man; he follows Ruskin and Geddes. Necessary discussions of sex life are given with admirable sincerity. One is a little surprised not to find references to Smuts whose 'Holism' has much kinship with Mumford's thought. Altogether one may congratulate the author on adding another stimulating book to the succession of studies of man's development by which he has done so much to promote thought.

H. J. FLEURE

**Fenno-Ugric Vocabulary: An Etymological Dictionary of the Uralic Languages.** By Björn Collinder. Uppsala (Almqvist & Wiksell), 1955. Pp. xxii, 212. Price 39 Sw. Kr.

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The term 'Uralic languages' seems to occur for the first time in a work by Klaproth about 135 years ago. It is now used to designate the community of the Finno-Ugric and the Samoyed languages. To the former family belong: A, (1) the Baltic Fennic languages: Finnish, Vote, Estonian, Livonian, (2) Lappish; B, the Volga Fennic languages: (1) Mordvin (Erza and Moksha), (2) Cheremis (in the vernacular Mari); C, the Permian languages (1) Votyak, (2) Ziryene; D, the Ugrian languages: (a) the Ob-Ugrian languages (1) Vogul (in the vernacular Mansi), (2) Ostyak (Hanti); (b) Hungarian (Magyar). The latter family is divided into the Northern and Southern Samoyed languages.

Scientific research on this language group was started by M. A. Castrén more than a century ago. Uralian linguistics might at present be considered one of the most successfully studied branches of linguistic science. The monographs have mostly been published in German or even in the vernaculars, the latter being especially the case as to the (at present uncompleted) etymological vocabularies of Finnish and Hungarian (the old comparative dictionary of Donner lacks practical value).

The initiative of Dr. Collinder in publishing this etymological vocabulary and doing it in English is therefore to be greeted with special gratitude. Its material is divided in three chapters: (A) Uralic Word List, pp. 1-69, (B) Fenno-Ugric Word List, pp. 70-127, (C) Indo-European Loan Words, pp. 128-141. The author

being a partisan of the Uralo-Altaic theory, several etymologies intended to support this theory are listed in an Appendix, pp. 142-149.

The greater part of the etymologies presented has been compiled from the literature about these languages. The text totally lacks any source references, and the quoted works are listed in the bibliography, pp. 205-212, which follows the Index, pp. 151-204. The author has of course tried to give only the most convincing etymologies, but the selection should have been made with a more rigorous hand. Detailed reviews of the Uralian and Finno-Ugrian sections have been published, e.g., by Mr. E. Itkonen (in *Ural-altaische Jahrbücher*, Vol. XXVIII, pp. 56-80) and Mr. A. Joki (in *Finnisch-ugrische Forschungen*, Vol. XXXII, Anzeiger, pp. 42-55). The *Survey of the Uralic Languages*, compiled by Björn Collinder, Uppsala (Almqvist & Wiksell), 1957, contains on pp. 517-536 an Appendix to *Fenno-Ugric Vocabulary*, in which several corrections are made.

Some further remarks are perhaps not inappropriate: p. 66 fi. *vaski*, 'copper, bronze': cf. also Tokhar. A *wās*, Armen. *oski*, 'gold,' connected by Zimmern with Sumerian *guš-kin*, id.; p. 115 fi. *suoja*, 'shelter': Ramstedt compared with Sanskr. *chāyā*, 'shadow'; Dr. Collinder has previously published a detailed study about the supposed affinity between the Uralian and Indo-European families, but in this vocabulary no attention seems to be paid to this theory, and etymologies earlier used to support it are here listed among the Indo-European loan words, which are presented alphabetically without any historical or phonetic classification. Those less versed in Uralian linguistics might be puzzled by combinations like (p. 130) fi. *marmaa*, 'dead' ~ Sanskr. *marta*, id. beside (p. 140) fi. *vermen*, 'epidermis,' Sanskr. *varman*, 'armour,' etc.; p. 135, *mordv*. E. *ravo*, *rav*, 'sea, the Volga river': the next Indo-European point of connexion seems to be the Scythian name of the river Volga, in its Greek form *Rha*; p. 138 fi. *talas*, *tala*, 'shed for a boat, nets, etc.' ~ Sanskr. *tala*, 'surface, plain, sole, etc.': cf. Mongol *tala*, 'plain, steppe'; p. 143 Nr. 8 fi. *e*: the Altaic pronominal stems *e*- and *i*- have been mixed up; why among the Mongolian languages is only the very modern and far developed Buryat quoted?; p. 155 Nr. 20 Mongol *hana*, 'where': why *h*- while elsewhere it is correctly given as *q*-?; Nr. 24 fi. *me*, 'we' ~ Mongol *bide*: the Mongol form to be compared would be the old exclusive pronoun *ba*, Gen. *manu*; Nr. 28 fi. *nuole*, 'to lick' ~ Osman, etc. *jala*, id.: Turkic *j*- here obviously goes back to a *d*-, as shown by Tungus *dala*-, Mongol *\*dalu* > *dolu*-, 'to lick'; Nr. 31: the etymology of Zakharov is most probably of no value; p. 146 Nr. 37 fi. *sinä*, 'thou' ~ Mongol *ši*, id.: the latter form is again a late Buryat development from *ši* < \**ti*; Nr. 39 fi. *sydän*, 'heart', has nothing to do with Tungus *seleme*, which is an adjectivum materiae in -ma from the noun *sele* 'iron,' the meaning 'heart' of *seleme* being a fiction of certain lexicographers; the same word occurs in Mongolian and Manchu: *seleme*, 'rapier, sword.'

PENTTI AALTO

**Les Colloques de Wégimont: Cercle international d'Études ethno-musicologiques.** Edited by Paul Collaer. Brussels (Elsevier), 1956. Pp. 234. Price 295 Belgian francs

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The field of research—or the variety of research activities—which is now usually called ethnomusicology has attracted more attention on the continent and across the Atlantic than in England. As a matter of fact, Vienna, Paris and Berlin began their systematic collections of recordings of non-European types of music (with which this new discipline was—and still is—primarily concerned) round about the turn of the century. It is a well-known fact that the principles formulated by Alexander Ellis (1814-90) form the basis of much of the work of the later scholars in Germany and elsewhere, but they never led to the foundation of a school or of institutes officially connected with Universities in this country. Figures like Fox Strangways or Henry Balfour remained more or less in isolation in contrast with, for instance, the group of workers in Berlin (especially von Hornbostel, Sachs, etc.) and their pupils.

It had long been apparent to bilingual or trilingual observers that much of the work done in the different countries was in many ways mutually complementary and sometimes overlapping, and it



seemed that a small international meeting place, where the different scholars could quietly examine each other's experiences and opinions, was urgently needed, apart from and in addition to the many large and hectic international conferences held periodically in different countries.

Owing to the energy of M. Paul Collaer (whose wide experience in the field of folk music in all parts of the world in a way predestined him for this task) and the co-operation of the University of Liège and the highest authorities of the Province of Liège, the Château de Wégimont was chosen for such a meeting and the publication under review largely consists of the texts of the papers read on that occasion. Over and above that, however, it contains some ten pages of most valuable and searching reviews of recent publications on the subject and a survey of ethnomusicological activities in 1955, such as research expeditions to different parts of the world. Finally also no less than eight full pages of commercial and non-commercial recordings from every part of the globe are listed.

Practically every item in this volume touches a vital aspect of this new discipline which, as Paul Collaer points out in his review of Marius Schneider's *Singende Steine*, can only exist in co-operation with other disciplines such as anthropology, physiology, ethnology, sociology, prehistory and, last but not least, psychology. His own contribution to the main body of the work touches on the physiological side of ethnomusicology by investigating to what extent the knowledge of the inherent qualities of the ear and the sound-producing organs can help to elucidate the laws governing the development of music in its initial stages. His 'État actuel des connaissances relatives à la perception auditive, etc.' is a stimulating and valuable contribution.

Marius Schneider touches the other extreme of the subject in his 'Fondements intellectuels et psychologiques du chant magique,' equally stimulating, though in some respects perhaps more controversial.

It is impossible to go into details, as the different contributions range through the whole of this wide field, each bringing out points worth serious consideration.

It is highly gratifying that the results of this first meeting at Wégimont have justified the organizers in making the *Colloques* a yearly feature. If maintained at this level, the publication of their proceedings will be—as the organizers hope—a worthy successor to the *Sammelbände für vergleichende Musikwissenschaft*. Hitherto there has been nothing to fill the gap left by the cessation of that remarkable series in 1923.

A. A. BAKE

**Mysticism: Sacred and Profane.** By R. C. Zaehner. Oxford (Clarendon Press: O.U.P.), 1957. Pp. xviii, 256. Price £2 2s.

This is a book about mysticism written by a scholar who is not himself a mystic. It is thus an academic work somewhat in the tradition of William James but with this difference, that we are now in an age of psychiatry and incipient knowledge of some of the workings of the human psyche, which the author alludes to.

The two opening chapters on 'Mescaline' are a welcome and high-spirited attack on Aldous Huxley's contention that the visionary impressions induced by this and allied drugs are related in any effective way to real religious experience. This is all to the good, but it is counterbalanced by the author's own confession (in both senses of the word) of his own Roman Catholic faith, which both sustains him and somewhat restricts his view. The authority of a direct drug-induced impact is replaced by another authoritative system which has indeed a long tradition behind it but is itself in need of re-interpretation, such as it cannot be said that Zaehner achieves or seems particularly interested in. He is, however, liberal enough to review much of current interest. His third chapter deals with 'Some Nature Mystics,' in which he ranges from *The Cloud of Unknowing* to Coomaraswami, René Guénon, Schuon and the *Védānta* and to works such as Jeffries's *The Story of my Heart*. This leads on to 'God or Nature? (Proust and Rimbaud)' and somewhat more questionably to the next chapter on 'Madness' in which he cites the manic-depressive John Cusance as an instance of one who has had glimpses of two opposite kinds of 'absolute.' But, since these have not been

integrated, Professor Zaehner has somewhat overvalued this testimony. Indeed where Zaehner touches psychology, his knowledge is more academic than practical. This is in spite of the fact that, in the words of the publisher's blurb, 'Having established the types [three types of mysticism which he distinguishes] he seeks further to define the relationship existing between them and, by making cautious use of Jungian psychology, to demonstrate the aptness of the Christian doctrines of the Resurrection and Ascension as well as that of the Holy Trinity.' In my opinion, he does no such thing.

The above will not much interest anthropologists, nor will the following chapter on 'Integration and Isolation'. What is however of real academic worth is the author's lucid exposition of Vedantic concepts and terminology in the next chapter on 'Some Hindu Approaches,' which he attempts to correlate with some of the findings of Jung and of his follower Erich Neumann in relation to what the latter calls 'uroboric incest.' In his discussion he cites Ramakrishna in the East and Angelus Silesius in the West, and makes much use of the *Upanishads* and the *Bhavad-gita* in comparison with some of the deeper Christian mystical writings such as those of St. John of the Cross. He recognizes much of the sexual symbolism common to all these, though in his comments on it he is not entirely free from a certain prudery.

The final chapters are on Theism versus Monism. In his Conclusion he says: 'In the course of this book we have tried to investigate the truth of the assertion that "mysticism" is an unvarying phenomenon observable throughout the entire world and in all ages, and that it may (and does) make its appearance in all and any religious system. . . . Our investigations have led to the tentative conclusion that what goes by the name of mysticism, so far from being an identical expression of the selfsame Universal Spirit, falls into three distinct categories. . . . Because these experiences are recorded at all times and from all parts of the world, it is fatally easy to assume that because they are, one and all, preternatural, that is, not explicable in the present state of our knowledge, and because the keynotes of all of them is "union," they must necessarily be the same. It is not realized often enough that once these experiences are assumed to be identical and of identical provenance, the conclusion that the transports of the saint and the ecstasies of the manic are identical cannot be escaped. If this were really so, and if these preternatural experiences were what religion is principally concerned with, then the only sensible course to adopt would be that which Rimbaud followed: we should all attempt to induce in ourselves an attack of acute mania; and this is in fact the solution that Mr. Huxley seems to propound in *The Doors of Perception*.' Zaehner himself tried mescaline (as described in Appendix B), but with abortive result.

Appendix C is a transliteration of passages from an Arabic treatise on mysticism by the Muslim mystic, Abū'l Qāsim al-Qushayrī, who flourished in the second half of the eleventh century of the Christian era, which have been rendered in English earlier in the book. Zaehner's reputation as a philologist is not in question.

This book, over and above its academic erudition, is written with verve and with an enthusiasm which carries the reader along but at the same time leads to some repetition. For those unfamiliar with these subjects it may be an eye-opener. For those more familiar, though it is rich in a certain type of symbolic thinking, it lacks the vision of personal experience which might indicate what mysticism is ultimately about, or what effect it has had in the development of human culture.

JOHN LAYARD

**The Aegean and the Near East: Studies Presented to Hetty Goldman on the Occasion of her Seventy-fifth Birthday.** Edited by Saul L. Weinberg. Locust Valley, N.Y. (Augustin), 1957. Pp. xvi, 322. Price \$10

This *Festschrift* has clearly been planned with skill and care to span very exactly the range of Miss Goldman's own interests, as illustrated in the selected bibliography of her writing. It is unfortunate, however, that, since many of these 22 essays are concerned with commercial and cultural contacts at various periods across the Levant, the book generally shows such an inadequate appreciation of topography. Ehrich's essay on culture area and culture history makes an ingenious academic exercise, but it is rather unrealistic, and



misleading in many respects; the 'Diyarbakir basin,' for example, is manifestly not an extension of the Syrian steppe (p. 15). Again, Blegen's argument, that in the third and second millennia sea trade round Anatolia was more important than land trade across it, is severely weakened by his erroneous description of the terrain. In fact, Cyrus the Younger did not follow the Royal Road (p. 32) but the Great Eastern Trade Route. Neither of these highways, however, passed through 'the rugged heavily-wooded fastnesses of Anatolia' (p. 34), but from oasis to oasis across the perfectly flat margins of the central plateau, which is still, as in the time of Strabo, *ἄξυλον*.

In a closely reasoned article, Barnett shows that he has come in the last few years closer to the view of Akurgal, that archaic Greece met the Orient on the Orontes rather than by the Lesser Mæander; and he adds important evidence for an eighth-century trade route from Elam through Urartu to Trebizond. Amandry's article on the Urartian bronze bulls, Miss Grace's on the Canaanite Jar, and that of the Weinbergs on Arachne of Lydia at Corinth present apt illustrations of Barnett's thesis, while Gordon's important comparisons between the mobile Ugaritic guilds and the Homeric *δημιουργοί* show one way in which techniques and ideas may have travelled. Mellink, starting from a study of the royal tombs of Alaca, presents some stimulating speculations about early Indo-European migrations and metal trade in the third millennium. It is, however, slightly

disturbing that while he takes the Cycladic 'frying-pans' as mirrors (p. 53), Barnett (p. 222) regards them as *sistra*.

Wace makes a powerful plea for dispelling the gloom of the Dark Age in the Aegean, and Albright, in a somewhat partisan essay, attempts to do the same for North Syria. Neither is completely convincing; even if the Mycenaeans did speak Greek, the change of script before classical times seems in this context more significant than the continuity of language. Braidwood's summary of recent work on neolithic beginnings in the Levant is thorough, though disappointingly inconclusive. Haspels deduces rather too much from the Seljuk sack of Dorylaeum, for over the Anatolian countryside generally the conquest was not so subversive as in western Phrygia. Benson contributes a scholarly article on archaic seal-impressions on Late Bronze Age Cypriot wares, and Koyan on the Alaca jewellery of the Early Bronze Age. There are two studies on Mycenaean terra-cotta figurines, three on finds from Tarsus, and three on classical topics—Hipparchus, a Marathon epigram from the Agora, and the Persian spoils in Athens.

The book thus includes much important new work, and in general gives a broad and welcome impression of the present state of scholarship in the fields where Miss Goldman has herself contributed so much fine and devoted work.

W. C. BRICE

## AMERICA

**Peru.** By G. H. S. Bushnell. London (Thames & Hudson), 1956. Pp. 207, 71 plates, 13 line drawings, bibl., index. Price £1 15.

**200** This volume is the first of a series 'Ancient Peoples and Places,' which is planned under the editorship of Mrs. Glyn Daniel. The series is intended to give the public up-to-date accounts of the present knowledge of the cultural development and status of certain ancient nations and countries which have been selected for presentation on the grounds of their general appeal to the public. If the choice of authors of the coming volumes is going to be as happy as in the present case, there is ample reason for congratulating the editor.

In its general outlines, Dr. Bushnell's book follows the frames established by Bennett and Bird in *Andean Culture History* (Handbook Series, No. 15, American Museum of Natural History, 1949), and especially Bennett's Part 2 of that book, with its grouping of the successive cultures into Early Farmers, Cultists, Experimenters, etc. This grouping having once been introduced, and apparently adopted by archaeologists generally, to judge from literature which has appeared since 1949, one cannot blame the author for following it, though to me it appears simpler to follow the grouping which was used by Bennett in 1946 (*Handbook of South American Indians*, Vol. III) where the greater periods, and more important ones, were called simply Early, Middle, and Late. By the introduction of such terms as Cultists, Mastercraftsmen, etc., Peruvian archaeology has become loaded with an extra set of quite superfluous designations of periods for which satisfactory and more neutral terms have already been established. And embarrassing it is that the new terms do not in every case characterize the periods which they stand for in a way that cannot be misunderstood. For instance, the term 'experimenter' cannot with any right be reserved for the period covering the last half-millennium before Christ, as experimenting must certainly have been going on throughout the whole span of time during which Peruvian cultures have developed. Similarly, the title of 'imperialist' which has been reserved for the Inca period, may equally well be applied to the Chimú, and perhaps to the Mochica as well as to other cultures of which our knowledge is insufficient at the present time. Perhaps this is too conservative a view, but actually it is my hope that the entire group of titles applied to the periods after the Early Farmers and upwards, a total of 10 artificial and unnecessary terms, may be allowed to pass out quietly again from the literature.

But, as I have said, Dr. Bushnell is not responsible for the titles, nor can he be blamed for using them, since they exist in the archaeological literature. His exposure of the successive cultures of the area is well balanced as a whole. He has not yielded to the temptation

of overdoing the Inca period and has thereby avoided the top-heaviness which disfigures so many popular accounts of the ancient Peruvian cultures. Most of the periods are treated on an equal footing, e.g. the Salinar culture, which in recent time has received much attention, has got the respect that is due to it; though one feels inclined to regret that the splendid Recuay ceramics have been disposed of in eight lines only. For the Chancay the author introduces the name of Cuismancu, and presents good reasons for doing so; but is it not a pity to discard a name which has served so well through all the years in which archaeologists have been dealing seriously with Peru?

The photographic plates at the end of the book are excellent. They demonstrate how important it is that discrimination is lavished on the selection of specimens to be shown, and competent photographic art employed, too. A couple of specimens from Sr. Larco Hoyle's collection (Bushnell, plates 4 and 12) have in this rendering acquired a life which one would hardly expect them to possess from the pictures in the *Handbook of South American Indians* where the same objects are shown in Vol. III, plates 64c and 67i.

Well written and well illustrated as it is, Dr. Bushnell's book ought to be received with acclamation by everybody who takes an interest in the cultures of ancient Peru. The author takes all the material into consideration which has been published until now, sorts out the roundabout talk which has been presented within the last 50 years during which Peruvian archaeology has been in the foreground, and in appropriate places gives brief references to the most recent contributions, such as the results of radio-carbon dating. The book forms a clear, condensed, and up-to-date introduction to the study of Peruvian archaeology; and at the same time a handy book of reference for those who have penetrated a little deeper into the subject.

JENS YDE

**Pre-Columbian Art: The Robert Woods Bliss Collection.**

Text by S. K. Lothrop, W. F. Foshag and Joy Mahler. London (Phaidon), 1957. Pp. 285, 270 illus. (165 in colour), 31 text figs., 3 maps. £7 7s.

**201** It should be remarked at the very outset that this volume is concerned with a rather specialized aspect of Pre-Columbian art: more than half of the objects illustrated in it are jades (or other semi-precious stones) and another quarter of the pieces are of gold or gold alloy. There are examples of Olmec (La Venta), Classic Vera Cruz (Tajín or Totonac), Teotihuacan, Aztec, Zapotec, Mixtec, Classic and Late Classic Maya, Nicoya peninsula, Veraguas, Coclé, Chibcha, Quimbaya, Cupisnique, Mochica, Nazca, Chimú,



Inca and other arts. Some ceramics, about thirty Peruvian textiles and a few miscellaneous objects occupy the remainder of the illustrations.

This book adequately discharges Mr. Bliss's expressed aim of making his excellent collection accessible to the public at large. In addition to photographs of more than 370 specimens, it provides a clear-cut summary of the major cultures and styles, some remarks on mineralogical attributions, brief technical discussions of textiles and metallurgy and a *catalogue raisonné*. Combining, as it does, sumptuousness of illustration with a modest text, and supported by the Phaidon prestige, *Pre-Columbian Art* should enjoy considerable popular success. From the viewpoint of scholars, the value of a book which offers so rich a glimpse of the *Kleinkunst* of Meso- and South America undoubtedly lies in the mere reproduction of the pieces.

There are of course, as in any book based on a single collection, certain weaknesses. Catalogue numbers 126 and 16-A ought definitely to be reconsidered, while the importance of several objects, despite the photographer's art, seems to have been somewhat overestimated. The chief shortcoming of the book, so far as I can see, is in the annotated catalogue at the end. This is little more than a description of the pieces (with a few comparisons and notes on interpretation and provenance) interspersed with some black-and-white plates apparently made from colour photographs. In contrast with previous Phaidon publications which have a unique authority in the fields of European and Asian art, the present volume contributes relatively little to the scholarly perception of Pre-Columbian art.

DOUGLAS F. FRASER

**De Azteken.** By Guda E. G. van Giffen-Duyvis. Amsterdam (Uitgeverij Maatschappij 'Holland'), 1957. Pp. 208, 62 plates, text figs., 2 maps

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Since the days when she visited the Americas during the twenties as an assistant to Walter Lehmann, Mrs. van Giffen has devoted much of her attention to the archaeology of Mexico and Peru. As a private scholar she has since been asked on several occasions to arrange exhibitions of pre-Columbian art in the Netherlands, for example one on Peruvian art, held in 1954 in the Central Museum in Utrecht, for which she managed to bring together many very good and little-known pieces, as well as prepared the excellent catalogue. Her contribution to the promotion of knowledge of pre-Columbian art and archaeology in the Netherlands, however, has not been restricted to the arrangement of exhibitions. The chapter which she wrote in 1951 on this subject in Volume VI of the *Algemene Kunstgeschiedenis* edited by Professor van Thienen, for example, is to be considered as a good introduction and as such well worth reading.

In her latest publication Mrs. van Giffen has restricted herself to the Aztecs. If the art of popularization of science is to be considered a difficult one (and in my opinion indeed it is), it has to be said that Mrs. van Giffen has succeeded very well. In nine chapters she gives a very readable account of the complicated history of the Aztecs, of the political organization, the means of subsistence, the social organization, education, religion, writing, the calendar system, and the arts, to conclude in the tenth chapter with a short outline of the cultural relations in the Mexican area. The text is well illustrated by many line drawings and 62 plates. Two maps at the end of the book explain the expansion of the Aztec empire, and the cultural relations in the Mexican area.

The specialist will find nothing new or revolutionary in this book, as it is based on the well-known standard literature in this field, of which a fairly complete list is given. The author, however, did not write for the specialist, but for the layman. From this point of view it is much more important to put on record that no errors or misinterpretations, not to speak of false exoticism, misguide the interested layman on his first steps in the fascinating field of Aztec history and archaeology. Of course there always remains something to be desired: one would have preferred the references to the illustrations incorporated in the text rather than brought together in lists at the end of the book; also, although the many foreign names and terms are explained at their proper place in the text, a glossary would have been useful for quick reference. But these are only

comments of minor importance on a publication upon which the author in the main is to be complimented.

A. A. GERBRANDS

**Andean Excavations.** By Stig Rydén. *Statens Etnografiska Museum. Monog. Ser. No. 4.* Stockholm (S.E.M.), 1957. Pp. 199, plates, text figs. Price \$10

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In this publication, Dr. Rydén describes his work in 1951-2 in an area some 50 kilometres north-east of Lake Titicaca, and in working here he follows a Swedish tradition, since Norden-skiöld studied the Pelechuco district, slightly further east, some 50 years ago. It is a bleak, uncomfortable area, in which the sites studied lie at 10,000 to 13,000 feet above sea level, and it is not surprising that it proves to have been a marginal one in antiquity.

Dr. Rydén studied two main groups of sites, and a single one, Ayaya, lying roughly between them. The first group, near Mollo, is some 40 to 50 kilometres south-east of the Peruvian border, and the work there consisted mainly of the excavation of graves, since few habitation sites in a fit state for stratigraphical tests were located. Such refuse deposits as could be excavated showed Decadent Tiahuanaco influence at the bottom, and that of the Inca at the top. This was confirmed by the study of the graves, which consisted of slab-lined subterranean cists of a rather uniform character, containing remains of huddled burials, with pottery covering much the same age range as the refuse deposits, with some types belonging apparently to a time between the fading of Tiahuanaco influence and the arrival of that of the Inca. A few bronze objects, mainly *tupos* or shawl pins of a type still used by Andean women, were found in the graves; they had a generally uniform composition (Cu 87 per cent., Sn 10 per cent., Ag 2 per cent., Pb 1 per cent.). Another feature of interest was evidence of fires in the graves; in several cases, a sherd had apparently been placed on top of the embers to conserve the heat as long as possible for the benefit of the deceased.

There was only time to reconnoitre the other group of sites, which was further to the north-west, only about 15 kilometres from the Peruvian border, near the town of Charazani. Two of these were apparently fortified dwelling sites, situated in inaccessible places, and built of rough 'pirca' masonry of irregular stones. One of them, Turi, is described in several places as a city, but since its outside measurements are only about 15 by 30 metres on the plan, this is an over-statement. No evidence for their age was found, but it is conjectured that they were built as a response to the threat of the Inca invasion. They are associated with square grave houses, a sort of *chullpa*, similar to some found at Ayaya, where several had two stories and one had three. This type was still in use by Aymara Indians at the time of the Conquest, and the author thinks that it is related to the subterranean slab cists of the Mollo group of sites. For this and other reasons, all the sites studied are ascribed to the Aymara.

This is a painstaking study of a somewhat unrewarding area, which does not appear to have been occupied before rather late post-Classic times. Dr. Rydén was deprived of the possibility of getting some radio-carbon dates by the gross ill-treatment of his material by the authorities in La Paz, but apart from this he seems to have extracted all possible information from it. He was at least fortunate in being able to publish it so fully.

G. H. S. BUSHNELL

**The Peyote Religion: A Study in Indian-White Relations.** By J. S. Slotkin. Glencoe, Ill. (Free Press), 1956. Pp. vii, 195. Price \$4

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The present work deviates in several respects from the earlier standard book on the subject, La Barre's *The Peyote Cult* (1938). The latter is, as its author says, 'an integrated comparative treatment of the religion,' and the position of the author is that of a scientist at a distance, as it were, from his subject. Slotkin's book, on the other hand, reveals a vivid participation in the faith and ritual of Peyotism; indeed, he is himself a member of the Peyote Church. It is thus not surprising if the author presents facts that have not been noted down in the previous literature on Peyote. At the same time, the reader sometimes feels a little uneasy on account of the orthodox ardour by which the author asserts the Peyote case. In his preface he gives the background for his decision to write the book: he thought,



he says, that he owed it to the Peyotists to put his anthropological training at their disposal, and one purpose of the book is 'to present a documented exposition of Peyotism for Whites, from the Peyotist point of view.' Thus far we can read this book as a testimony on Peyote faith and as an interesting personal document.

But what about the value of this work as a source on Peyote religion? It must be stated that as a professional anthropologist Dr. Slotkin has at the same time been at pains to treat his subject from a scientific point of view. The book is less a description of Peyote religion as such than a study of Indian-White relations in connexion with the development of the Peyote religion. His thesis is that this religion is 'an Indian defense against consequences of White domination,' and 'that Peyotism socially is an example of accommodation rather than militancy; culturally, that it is a case of Pan-Indian nativism.' The author demonstrates this thesis against the background of recent American Indian history, especially, of course, on the Plains. In this connexion he discriminates between the old Peyote complex, a 'religion-like rite' which was an intermediate form, and the Peyote religion. The historical interpretation needs a certain revision after the publication of Howard's paper on the Mescal Bean Cult (see *Amer. Anthropol.*, Vol. LIX, 1957, pp. 75ff.). This revision applies first and foremost to the origins of the Peyote religion, which might have to do with the Southern Plains Bean cult.

The Peyote religion is defined as 'a trait complex consisting of voluntary association whose rite is one of singing, prayer and quiet contemplation, centered on Peyote both as a symbol of the spirits being worshipped and as a sacrament' (p. 23). The outline of the cult and its requisites is informative and illuminating. However, certain points could have been stressed more thoroughly. For instance, one would like to know more details about the process of the establishing of the Union Church. Some references to the reactions of Peyotists to their old tribal religion would have been of interest; a very common idea is that there was no 'true' religion before Peyotism, that the pre-Peyotists were 'heathens.' Indeed, I have found many Peyotists asserting that there was no eschatological belief at all before the introduction of Peyotism. (By the way, the author gives too simplified a picture of ancient Plains eschatology.) Surprisingly enough, the author does not mention the importance of Mother Earth in connexion with modern Peyote mythology and cult.

The book is very compact, a concise manual of Peyote religion, more than half of its space being filled with notes and references. It is a good source book for anyone interested in the subject who is aware that it has its bias in the sense that it is written for the Whites from the Peyotist point of view.

ÅKE HULTKRANTZ

**The Structure of a Moral Code: A Philosophical Analysis of Ethical Discourse Applied to the Ethics of the Navaho Indians.** By John Ladd. Cambridge, Mass. (Harvard U.P.) (London agents: O.U.P.), 1957. Pp. xviii, 474. Price £3 3s.

Professor Ladd's book is a welcome addition to the small but growing body of work by philosophers who take a serious interest in anthropology. It can be read by philosophers as an essay in moral philosophy in its own right, which puts forward a particular view about the nature of moral codes, but tests it not simply, as is current fashion, against the moral discourse of Anglo-American society, but against the moral discourse of a non-literate people—or rather, of one of their wise old men. The latter's views are taken boldly as giving the essentials of Navaho moral beliefs. Anthropologists may think that this means that the field data are thin, but Professor Ladd forearms himself against this criticism by putting his conclusions forward merely as hypotheses to be further tested by those in a position to make fuller enquiries. His method is to start by specifying carefully what one should be looking for in looking for a moral code. The distinctions he draws for this, with the help of recent work in the logic of ethics, may be helpful to any anthropologists who may feel that 'morality' is often used vaguely as a blanket term to cover a good many different aspects of conduct. Professor Ladd narrows it down—some may think unduly—to cover such injunctions and prohibitions as are accepted as guides to conduct. And not all of these, nor as guiding all conduct; where moral prescriptions

apply, these are (1) marked by claims to superiority and (2) regarded as legitimate. (1) means that where they apply, they are thought to have priority; (2) means that reasons are adduced to validate them. These are grounds accepted as good grounds by those giving them; they need not be premises from which moral statements are deduced, but are more likely to be what Mill calls considerations capable of influencing the intellect.

The study is said to be one in descriptive ethics, i.e. the analysis of what is to count as a moral code is taken as preliminary to finding out what such a code is in any given society. This is then applied to Navaho pronouncements on morals, and the conclusion drawn that Navaho hold an ethics of prudential materialism, rather like that of Hobbes. Actions are enjoined or proscribed because of consequences for individual wellbeing; social actions are seen as contributory to this. The evidence cited for this is drawn exclusively from spoken discourse. Ladd adopts this restriction deliberately; he holds that to discover a moral code you must attend to what people say and not observe what they do, since they will often sin and fall short of their codes. A code is a list of prescriptions, not a description of actual behaviour. A behaviourist account cannot be an account of principles which may be more honoured in the breach than the observance. While it may be a good corrective to point this out, surely the answers to direct questions given by a few informants need to be supplemented by observing comments and criticisms on behaviour made spontaneously in actual situations; this of course calls for more extensive field work and knowledge of the language. Professor Ladd would no doubt admit this; the value of his book is not as an alternative to such field work, but as suggesting questions which field anthropologists might have in mind and distinctions which they might draw.

On one point British social anthropologists are likely to want to protest. Professor Ladd thinks that before the coming of the U.S. Administration the Navaho would have been living in a Hobbesian state of nature. Work such as that of Evans-Pritchard and Fortes on the political and Gluckman on the legal systems of non-literate peoples surely makes it evident that the absence of formal administration does not mean absence of procedures for attending to common arrangements, and procedures which are in a genuine sense legal and political.

DOROTHY EMMET

**The Gros Ventres of Montana: Pt. II, Religion and Ritual.** By John M. Cooper, edited by Regina Flannery. Washington, D.C. (Catholic U. of America P.), 1956. Pp. 491. Price \$5

This detailed account of Gros Ventre religious belief and practice adheres closely to data as collected from informants. Although many of the Gros Ventres are now practising Catholics, reverence for the old beliefs has not died out. One woman, for instance, knelt and prayed to the Indian Supreme Being before she told about the Feathered Pipe, for, as she said, 'this Pipe was given from heaven' (p. 130). It is typical of the author's and editor's approach that in citing this incident they use an initial capital for 'Pipe' but not for 'heaven.' Throughout the volume there is strict adherence to the Gros Ventre point of view.

Many of the details of Gros Ventre ritual are gone and present-day beliefs must always be open to the suspicion of influence from Christianity. Nevertheless, the essential character of the religion may be accepted as validly Gros Ventre in character if for no other reason than that it so drastically departs from Christianity. If the informants did not get these ideas from contact with the old religion, where could they possibly have obtained them? It is only for rituals which are no longer performed that they must search their memories and quote their fathers and grandfathers. For many beliefs, as that in the power of thought (pp. 365-70), the difficulty lies rather in communicating to anthropologists of alien background what was previously inarticulate. The raw materials for understanding Plains religion are fairly copious and the consistency of pattern as evidenced by early sources is striking. Yet there are considerable difficulties to the communication of the anthropologists' understanding to the non-specialist. The present book makes no effort in this direction but it supplies important detail of the type without which further attempts could not even be contemplated.

MARIAN W. SMITH







(a)



(b)



**PAINTINGS OF CHARIOTS AT MORHANA PAHAR, INDIA**

*Photographs: Mrs. B. Allchin*



# MORHANA PAHAR: A REDISCOVERY\*

by

MRS. BRIDGET ALLCHIN, PH.D.

**207** The caves known as Morhana Pahar must have been first discovered by A. C. Carlleyle in 1880 or 1881, while he was First Assistant, Archaeological Survey of India, under General Cunningham. He himself makes only a passing reference to discoveries made in the general area of south Mirzapur and Rewa districts in a report published in 1885.<sup>1</sup> In 1899 his colleague, J. Cockburn, writing after Carlleyle's death, states<sup>2</sup> that he knew that Carlleyle had made some important discoveries during his tours 'but, as he desired to work them himself, he imparted no information on either the nature or the localities of the discoveries, and his knowledge has died with him.'

This, however, is only partly true. One of Carlleyle's executors, Charles Seidler, sent selections of his finds, which consisted of stone artifacts, chiefly microliths, to the British Museum, to the United States National Museum, to the national museums of Scotland and Ireland, to Oxford and to museums on the continent. In some cases the artifacts were accompanied by extracts from Carlleyle's notes and lists of sites. These were later quoted by several writers who described the collections received in their museums, notably Thomas Wilson of Washington<sup>3</sup> and George Black of Edinburgh,<sup>4</sup> and also by writers in Dublin and Brussels. Incidentally, the fact that this happened in several cases suggests that many more of his finds may have passed into obscurity.

The fact that many of the names in the lists of sites quoted by these writers corresponded with those written not only on artifacts known to have come to museums from Carlleyle *via* Seidler, but also on a large number included in other collections, notably the Sturge collection, which came to the British Museum after Carlleyle's death, suggested to my husband and myself, when we were going through this material in 1955, that these particular finds were also part of the Carlleyle collections which had found their way into the British Museum at second hand. Further investigations established this almost certainly, for not only were the tools themselves similar in all respects to those from collections registered as coming from Carlleyle, but the whole range of site names corresponded to those in the lists. Indeed it seems highly probable that the writing on the tools and the brief accompanying notes are in Carlleyle's own hand. K. R. U. Todd was evidently quite unaware of the true origin of the stone artifacts, when he visited the British Museum shortly after the last war, for a note put in with some specimens from the Sturge collection at this time simply says that he thought they came from the Jubblepore region—which is more or less correct, and is no doubt based on the appearance of the material from which they were made.

The artifacts in Carlleyle's collections consist almost

\* With Plate M

entirely of microliths, geometric forms and small blades and flakes, all obviously highly selected. They are made of various kinds of chert and chalcedony, including many semi-precious stones. The artifacts are not further described here, as I intend to publish another paper in the near future describing them in detail.

In 1906 Vincent Smith, summarizing the evidence from different parts of the world regarding pygmy flints (as he called them), and their uses, described and illustrated some of Carlleyle's finds from London and Dublin.<sup>5</sup> He also quoted at some length from Carlleyle's notes which had been placed at his disposal by a common friend, the Rev. A. C. Gatty. Here Carlleyle described in general terms the nature of the caves, and the rock paintings and stone tools which they contained, but gave no specific details of their whereabouts. His description, however, when combined with the lists of sites, and the huge quantity of material which we were now in a position to ascribe to some of these sites, seemed to warrant making some effort to rediscover them.

A certain number of the sites were described as being near villages, but when we tried to find these on maps we found the same village names occurring repeatedly throughout Mirzapur and Rewa districts, while the names of caves and hills usually did not appear at all. None the less by process of elimination certain areas appeared more hopeful than others in which to look for Carlleyle's sites.

In the winter of 1957-8 I was able to spend several months in Mirzapur, Rewa and adjoining districts. In the course of my explorations I arrived at the village of Bhainswar (Bhainsod) 42 miles south of Mirzapur on the Great Deccan Road. Here I found that not only were Morhana Pahar and many more of the sites listed by Wilson and Vincent Smith well known, but even Carlleyle himself was remembered.

Morhana Pahar proved to be a group of caves or deep rock shelters which lie on the top of the escarpment of the Vindhya hills, overlooking the Ganges valley from the south. The caves are in Mirzapur district about five miles north of Hanmana village, and three miles west of the road from Mirzapur to Hanmana. They are formed by the dissolution of the lower strata of a number of great blocks of Vindhyan sandstone which stand in a group on the edge of the plateau. The surface of the plateau at this point has only a thin covering of soil. The dry rolling grassland is sparsely forested in places, and broken at intervals by further groups of sandstone rocks, like those of Morhana Pahar. Between some of these are tiny cultivated patches of *kodon* (a small variety of millet). The nearest habitation is several miles distant, and the area is used mainly for grazing cattle and collecting firewood. Immediately west of the caves a small permanent stream breaks through the plateau edge to the plain below, providing a convenient source of water.

The change at the edge of the escarpment is dramatic:



the land drops away steeply, and patches of bare rock and scree alternate with bamboo and forest trees which cling to the hillside. Over 700 feet below lies the intensely cultivated Ganges plain. Despite its apparent proximity to the plain Morhana Pahar is an isolated and unfrequented spot, for not only the escarpment, but an immense physical and cultural dichotomy divides the sparsely populated plateau edge from the plain below. Further south, where the soil is more productive, the life of the plains seems to reappear for a time in a modified form, but here on the northern edge of the Vindhya one is in a world apart, cut off from the main streams of human life today, and possibly also in the past.

Although such occupation deposits as there may once have been in the caves appeared to have been disturbed or removed, presumably by Carlleyle, both the caves and the area surrounding them were strewn with stone artifacts in all stages of completion, and with *débris* from their manufacture. Clearly they were the complement of the carefully selected blades and geometric forms in the museum collections. In all the habitable caves and shelters every available flat surface on the walls and ceiling has numerous paintings in shades of orange, red, purple and occasionally creamy white. In spite of an exhaustive search no objects of metal or of pecked or polished stone were to be found: a fact which had already been observed by Carlleyle.<sup>6</sup>

This fact alone makes the paintings in the most westerly of the caves extremely interesting. At Morhana Pahar, as in many other Central Indian caves and rock shelters the subject of the paintings is generally either hunting or dancing scenes, or simply wild animals grazing either singly or in groups. In this one cave, however, two horse-drawn chariots are depicted.

As is frequently the case when primitive people draw objects which they have seen, but whose use and construction they do not fully understand, they have a number of obvious inaccuracies, but there is no doubt what they are intended to be. The first chariot is drawn by two horses (Plate Ma) and the second by four (Plate Mb). The second chariot is being attacked by two men, one with a bow and arrow, the other with a shield and a short spear. In both cases the chariots are shown partly in side view, and partly in plan with the wheels spread out, and in both cases the horses are pulling the chariot by means of wooden yokes attached to a pole; only in the case of the second are the reins drawn. Both appear to be two-wheeled chariots with long axles, square floors and high fronts, behind which stand the charioteers. The first, however, may be intended to have another pair of wheels, as it is not clear whether the two circles shown one on each side near the front are intended to represent wheels, or the sides of the vehicle shown in plan. Both charioteers stand alone, the first carrying a disc (*chakra*) or round shield raised in his right hand, and the second a spear or trident (*trishūla*) in his left.

The whole scene had been originally executed in dark red, and afterwards carefully overlaid in white. It seems possible that this was done by Carlleyle in an attempt to make it easier to photograph.

There is one other scene which may be related to the chariots, on the roof of a more easterly cave in the same

group. It is unfortunately too faint to photograph, but shows a man on horseback with a long spear being attacked by five or six bowmen similar to the one in the chariot scene. Incidentally they have also many counterparts in the hunting scenes all around. The angle of the horseman's body suggests that he has been hit and is about to fall to the ground. These are the only representations of horses being either ridden or driven which I saw in the Bhainswar area, and indeed the only paintings there which suggest the presence of metal-using people.

The style and the original red colour of these drawings strongly suggest that they were done by the same people who executed the numerous hunting and dancing scenes all round. The presence of pieces of worn red and purple hæmatite in association with the stone tools was noted by both Carlleyle<sup>6</sup> and myself at this and many other sites. Carlleyle claimed to have found them associated in the deposit at a depth of one foot or over in a number of cases (all unspecified); I found them only on the surface, for among the many caves which I visited none appeared to warrant excavation. In all cases the occupation deposit, if any, was only a few inches deep. The tendency has been for soil to be eroded out of caves and rock shelters, but in many cases leaving heavier objects such as stone artifacts and hæmatite behind. While it is impossible to believe that all this denudation is due only to Carlleyle, particularly as I found a number of sites which are definitely not included in his lists, and his remarks about the depth of the deposits may therefore be exaggerated, the fact that our observations coincide regarding both the association of hæmatite and microliths and the total absence of metal objects, in spite of a systematic search being made for them, must have some significance.

Chariots are known to have been in use in India during early historical times, from the Aryan invasions to approximately the second century A.D., after which the evidence suggests that their use declined steadily.

Throughout India today we find people in adjacent regions living at totally different levels of technical and social development. The Chenchu, of the Godavari region, for instance, are hunting people who have continued their way of life virtually unchanged for hundreds of years, on the fringes of the settled areas which formed the feudal kingdoms of the Deccan. Similarly the Baiga still live by hunting and shifting agriculture in spite of their proximity to the more advanced and densely populated Ganges plain. It is highly probable therefore that such contrasts existed, and perhaps were even more marked, in the past.

In the light of these facts it is possible that the chariots of Morhana Pahar are a record of a sortie, most probably in the early centuries B.C., from some centre in the Ganges Jamuna Doab into the territory of hunting tribes who still used no metal. There are of course many other possibilities: the chariots may be the doodlings of a herd boy, which seems unlikely on stylistic grounds; they may be the work of a devotee of Surya, the sun god, who left no other trace of his presence in the caves. The latter suggestion would provide an explanation of the disc in the hand of the first charioteer as a sun symbol. Parallels can be found in chariots



of Surya on stone reliefs at Bodhgaya and other Buddhist sites, but they differ on iconographic and stylistic grounds, as the chariot of Surya is traditionally shown in front view, four horses advancing with the head of the god showing above theirs.

Whatever the explanation of the chariots, they are remarkable among Indian rock paintings, just as Morhana Pahar is remarkable among late stone-age sites. Its isolation, its spectacular setting, and the even more spectacular views which it commands of the plains below, together with the richness of its stone industry and its rock art must, understandably, have endeared it to Carlleyle. It is not surprising that 'he desired to work them himself.'

#### Acknowledgments

I should like to record here my gratitude to Mr. J. W. Brailsford and other members of the Department of British and Mediaeval

Antiquities in the British Museum for allowing me constant access to their reserve collections, and for much kind assistance. My fieldwork in Central India was made possible by a grant from the Research Fund of the University of London.

#### Notes

<sup>1</sup> A. C. Carlleyle, 'Report of Tours in Gorakpur and Ghazipur,' *Archaeol. Surv. Rep.*, XXII (1885).

<sup>2</sup> J. Cockburn, 'Cave Drawings in the Kaimur Range, North-West Province,' *J. R. Asiat. Soc.*, 1899, pp. 89-97.

<sup>3</sup> T. Wilson, 'Minute Stone Implements from India,' *Ann. Rep. Smiths. Inst., Rep. U.S. Nat. Mus.*, 1892, pp. 455-60.

<sup>4</sup> G. F. Black, 'Stone Implements from Asia and Africa,' *Proc. Scot. Antiq. Soc.*, 3rd ser., Vol. II, pp. 407-11.

<sup>5</sup> V. A. Smith, 'Pygmy Flints,' *Ind. Antiq.*, Vol. XXXV (1906), pp. 185-95.

<sup>6</sup> *Id.*, *ib.*, p. 187.

## A NOTE ON THE DURABILITY OF MALAY MARRIAGES

by

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**208** The Malay marriage tie<sup>1</sup> is generally regarded as being extremely fragile, and such figures as are available<sup>2</sup> support this common view, showing divorces to number about half the total of marriages in any year.

However, without adequate supplementary information it is impossible to go very far with the analysis of such broad figures and in this note attention will be confined to three areas for which the results of anthropological fieldwork are available. These are, first, the Kelantan coastal villages studied by Professor and Mrs. Firth<sup>3</sup>; secondly, urban and rural Singapore, studied by Dr. J. Djamour<sup>4</sup>; and finally my own observations from Negri Sembilan.<sup>5</sup>

The first two reports give a clear picture of marriage instability in the sense of the frequent rupture of the jural bond between husband and wife.<sup>6</sup>

For the State of Negri Sembilan as a whole divorce seems to be as frequent as in the rest of the country, but in the District where I was resident the situation was very different. Thus, of the 53 married women, of all ages, living in one village, only seven had ever been divorced, three of these, moreover, only from unions entered after the death of the first husband. This picture is confirmed by my knowledge of the rest of the District. However, this further information was unsystematically gathered, incidental to research into other problems, and I therefore refrain from producing it in numerical form.

Two points of distinction emerge: (1) marriages in this particular area of Negri Sembilan are much more durable than marriages in either Kelantan or Singapore, (2) where divorce does occur it is the unions between previously married people that are particularly fragile. Here too there seems to be a direct contrast with the situation in Kelantan.<sup>7</sup> This second statement is not based solely on the experience

of the seven women noted above as having been divorced, but emerges clearly from the marriage histories of other individuals that I was able to obtain.

The remainder of this note is devoted to an attempt to correlate these differences in marriage durability with differences in other elements of the social structure.

#### The Legal Framework

In any society the frequency of divorce is partly determined by the difficulty of obtaining one. Throughout Malaya, Malay divorce and marriage are bound by Islamic law. This allows a husband to divorce at will, while seeming to allow a woman almost no opportunity to obtain a divorce on her own initiative. In fact, it is widely recognized that a determined woman can easily drive her husband to divorce her out of exasperation, and in this she is aided by the popular notion that a man should be ashamed (*malu*) to refuse a divorce to a wife who no longer wants him.

In all three areas divorce is easily obtained by both men and women, and there is no significant variation in the law to 'explain' the difference in the durability of marriages.

#### Value Attitudes towards Divorce

Divorce carries no moral stigma. Nor is there any moral prestige attached to 'putting up with it' if a marriage has become unsatisfactory to either party. Divorce is regarded as just as normal as marriage, and as an ever-present possibility within any marriage. In short, the moral view of divorce corresponds very closely to that reported elsewhere, although this 'permitted' divorce figures so much more rarely as the outcome of domestic differences.



### Kinship Organization

In Kelantan and Singapore kinship is reckoned bilaterally, giving rise to egocentric groupings which are the same only for siblings of the same father and mother.

In Negri Sembilan kinship organization is by exogamous matrilineal descent groups of a corporate character. These groupings may be distinguished at clan, sub-clan and extended-family levels. The last two, and especially the last, are those important for the present discussion.

Within each village there may be observed groups of houses, which, with the land on which they stand, are the property and residence of groups of closely related women and their families. Owing to exogamy and the matrilineal residence rule, the adult male kin of this group will be living elsewhere, but there is still a sense in which their place is with their female kin rather than with their wives and children. Also in some contexts they are in control of these women and their offspring. With modern developments there has been a reduction in the extent of this control. Even so, for ceremonial purposes, and in a crisis where the individual needs assistance, it is with the matrilineal kin that the responsibility lies. This can be seen very clearly in the rituals of betrothal and marriage which give no part at all to the father and his kin.

In arranging marriages the preference is for someone 'near.' As cross-cousins are the nearest possible naturally a large number of these marriages occur (especially of the mother's brother's daughter with the father's sister's son). This preference, however, is not explicitly institutionalized; there is, for example, no terminological recognition of the relationship. In fact the nearness sought is social rather than a question solely of genealogy, and the nature of the relations between the respective parents may lead to the cross-cousin being passed over in favour of more remote kin, or even an unrelated person. However, it is usually possible to revive some dormant connexion to prove that the proposed *menantu* (son or daughter in law) is not, in fact, a stranger.

However remote the connexion may, on the face of it, appear to be, there will normally be one, and, moreover, one that is of practical importance in the concrete circumstances. If not, the contracting parties would not have come to make the match. From this it follows that there is usually at least one person strategically placed with an interest in both sides of the marriage. This is seen at its clearest when the father of the bride is the maternal uncle of the groom, and especially so, when, as often occurs, the marriages develop a sort of exchange pattern over time, sons-in-law being drawn from the same group as the bride's father, while sons go as *menantu* to that group.

The reason most often given for marrying 'near' is property: either the desire that wealth belonging to the parents should on their death benefit someone related rather than a stranger, or the sharing of poverty. This occurs when the father of the girl is poor and she is therefore a poor match; then it is common for him to put pressure on one of his young kinsmen to marry his daughter. Indeed, people often speak as though a man has the right to 'take' one of his kin in this way, whether the chosen candidate agree or not.

Apart from wealth, it is also thought that for the groom, and for the other residents of the house to which he will move on marriage, personal relations will be easier if he is not a complete stranger.

Finally, the very giving of one's son to another as *menantu* in itself constitutes a prestation. There is the actual transfer of his presence and earning power. More important, it allows the girl's parents to meet their very important obligation to see her married. Expressed as a 'debt' towards one's children this obligation weighs heavily with a Malay parent, above all to a daughter, and they see any delay beyond the normal marrying age as fraught with possibilities of *malu*. The providers of the new *menantu* enable the debt to be paid, and so confer a benefit upon his new affines; as the villagers say, it is better to look after one's own before helping strangers.

### The Implications of Divorce

In this section I wish to use two distinctions. The first is between the 'reactions of the kin group' to a divorce and the 'practical considerations' bearing on a rational decision by the individual on whether to divorce or not.

Secondly, I wish to distinguish two phases in the development of a marriage. The first is the 'adjustment period' which begins with the ceremony and ends as the new couple are gradually accepted as fully responsible members of the community. As a criterion of the end of this phase some reliance may be placed on the birth of children and the building of separate quarters for the new family, but there are important variations between individuals.

The second phase is that of 'full adulthood,' admittedly a very broad category, but precise enough for our purpose here.

(1) *The adjustment period.* Although all Malay youths accept the necessity of their marrying, and often welcome it, nevertheless there are disadvantages involved which are likely to weigh heavily with a young man. He has, first of all, to accept the authority of his *mentua* (wife's parents) and the whole *tempat semanda*.<sup>8</sup> Thanks to the pattern of preferential marriage this is less irksome than it might otherwise be, but it is still important, and generally recognized as such in the society. Secondly, the husband has to assume financial responsibility for the support of his wife. This contrasts strongly with his position within his parental household; although an adolescent boy can earn as much as a grown man tapping rubber he will be expected to make only small and irregular contributions to the family budget, retaining the rest of his earnings for his own use. Finally he will have to give up participating in the amusements of his still unmarried friends, partly because it is not proper and his *mentua* will object, partly because he can no longer afford it. But it is also likely that he will be married in another village, too far to mix as before with his former friends while the youth of his new village are, at best, remote in their attitude towards him.

The bride faces the problem of getting used to her husband. Although she is formally asked whether she accepts the choice made for her or not she has little freedom in the matter, and in many marriages the girl shows the greatest



difficulty in accepting her parents' choice of a spouse and the sexual aspects of her new role. Also the girl will have to work much harder when she is married. Although she will have helped her mother about the house prior to her marriage such work is not ordinarily arduous, and above all, it is not proper that an unmarried girl should work in the rice fields. After marriage, however, she will be expected to do a full share of the work, and the task of caring for her husband will be specifically hers.

Thus there are important disadvantages, as compared with single status, for the newly married. These can all be avoided through divorce. A man who is young enough can easily revert to the status of *budak-budak kampung* (lads of the village), going back to live in his mother's house, and mixing with his former friends, on the same terms as before. A girl, too, easily reverts to being the dependant of her father, and since the 'debt' has been paid she will be given much more freedom in the choice of any subsequent spouse.

It would therefore seem reasonable to expect a high incidence of divorce in the adjustment period, especially as, legally, divorce is easily obtainable, and value attitudes towards it are permissive.

Divorce, however, is not common, and it is here that the importance of the kin group is seen. An aspect of the indulgence that accepts that children should not work too hard, and allows sons to dispose freely of their earnings, is the view that the young are too irresponsible to manage their own affairs when it is a question of weight. The authority that attaches to age and seniority will be brought to bear if it seems that a divorce is likely to result through the 'foolishness' of the young. The marriage arranged is that which is felt to be the best possible for all concerned, and all possible suasion will be used by the families and kin of the principals to restrain them from divorce and the wrecking of the carefully laid plans, in the belief that later they too will appreciate the wisdom of the decision made for them after they have settled down.

As can be seen from the durability of first marriage this pressure is normally effective. Why then does it not occur, or not work, in other areas of Malaya?

Partly the answer lies in the greater effectiveness of unilineal groups, as against the kindred, in co-activity. Their corporate nature, and definite single membership give rise to sole loyalty and identification, and allow a more effective mobilization of pressure than is possible with the continual shifting according to context that must occur with groupings of the latter type. Again, the pattern of preferential marriage means that any one marriage is not the only tie between the groups, and if possible divorce is to be avoided for fear of ramifying bad relations.

A marriage is an agreement between the kin groups. At the betrothal a senior male of each extended family agrees, in the presence of witnesses, to the date, and other details, of the wedding. The betrothal implies the assent, at least tacit, of all members of the kin group, and they therefore share some responsibility for seeing that the agreement is carried out; and this means not only that the ceremony should take place, but that a satisfactory marriage should result. Participation in the wedding, and the actual rituals,

symbolizing the group character of the ceremony, have the same implications. Indeed, it was once necessary, and is still desirable, that all outstanding disputes and differences within the group be settled before a ceremony is carried out, in order that all members may participate.

(2) *The adult phase of a marriage.* In the case of an adult contemplating divorce the importance of the two influences of kin-group pressure and practical interest is reversed. An adult is left to manage his or her affairs without any 'official' pressure from the group. Moreover, should there be any wish to interfere an adult is not accessible to sanctions in the same way. A husband-wife quarrel will not generally spread into general bad relations between the respective kin. It is recognized that they cannot really be held responsible for the actions of an adult in the same way as they are for those of an adolescent relative, and a divorce after a couple have been married for some years does not constitute a breach of the agreement in the same way as divorce when the marriage has hardly started. Finally, if two adults decide to divorce it can hardly be dismissed as a temporary reaction to the change from single to married status to which the individual will soon become reconciled if restrained for a while from the initial resolution to divorce.

But a man who has been married to the same woman for a number of years will probably find the balance of practical interests so weighted that it will be only after the greatest provocation that he will divorce. Similarly with a woman.

House sites and rice fields in Negri Sembilan are generally the property of women. The house that a man builds on the site belonging to his wife he leaves with her should they divorce. This may well represent a considerable loss, while a newly married man, even if no longer living with his *mentua*, will probably have built only a bamboo hut representing the outlay of little more than his time.

With regard to the question of returning to the matrilineal kin too the situation is different. For the young, newly married man, it is simply a matter of reverting to the status of unmarried son in his mother's house. For an older man, even if his parents still be in a position to accept him, his status will be unsatisfactory for a man who has been 'master in his own house.' And in actual fact, for the older man divorce often means going to live in a sister's house, i.e. in a household headed by his brother-in-law. Although a man has inalienable rights in the ancestral property, the use and ownership of which is vested in his sisters, and although their house is the obvious place for him to go on divorce, or at the death of his wife, he may well find that his prolonged presence is regarded as an imposition rather than the enjoyment of a legitimate privilege.

Should a man have children, as yet unmarried, these will definitely stay with their mother, or with her kin (e.g. if the wife has died). For legal and religious purposes his status as father is irrevocable, and occasionally he may be able to have the children stay with him a while. But for all practical purposes they are as good as lost.

More generally, in a peasant society a man's economic interests are very much tied up with his place of residence, and this, of course, he must change immediately on divorce.



For a woman the consequences of divorce are likely to be even more serious. Should her father be able to support her he will, but for most women, especially those left with children, the prospect is one of severe economic hardship.

Other factors, such as the difficulty of a woman participating much in social events without a husband, might be detailed. But, of course, the continuation of a marriage that has in any case lasted for a good while is not really problematic, depending as it does on an awkward residual category that contains elements such as affection and inertia. These, in fact, are the things that people mention when, having spoken of divorce in anger, they change their minds on cooling. Only after reflection will more objective factors such as economic loss, or a dislike of lodging with the *ipar* (in-laws) be considered.

We turn now to the attitude of the kin group. As stated above it will not normally intervene to prevent a divorce or effect a reconciliation unless asked to do so. Only after a divorce is there intervention by kin and neighbours, and then their interest is rather to find a new spouse.

The institutions of Malay society assume that all adults are married. Should anyone revert to single status the possibilities of a new match are canvassed at once. Even should the individuals in question be in no particular hurry to marry again they will be subjected to continual advice, persuasions, and propositions until they relent. The pressure is particularly strong on a woman. First of all her economic situation will probably be very uncomfortable. But also, if she be at all personable, a *janda* (divorcée or widow) is considered to be in great moral danger; even if her conduct be unexceptionable her male kin will be criticized for not guarding their own and the village reputation properly if they delay in finding her a new husband. This is another situation in which the importance of *malu* can be clearly seen, and every effort will be made to get the woman married.

A wifeless man is much more independent than a woman, but he too will be faced with attempts to match him off. In part he has his own discomfort without a proper place to live to serve as an inducement; also there will be the efforts of his maternal kin who do not relish his prolonged presence in their house, but he will also be subject to the wiles of those who see in him a suitable match for a *janda* under their care.

Naturally marriages made under this sort of pressure are not particularly durable, and neither the kin nor the practical interests that grow up within a marriage can exert a binding influence.

Moreover, there is within these marriages an important contractual element, both parties seeking to gain by the division of labour by sexes. The man seeks a house, rice land, and the satisfactory performance of domestic work, the woman someone who will provide the cash income necessary for an adequate standard of living. In these circumstances, if either party be dissatisfied the obvious thing to do is divorce and try again, and that is often just what people do. The simplicity and cheapness of subsequent weddings dispose people to regard them lightly. A marriage

payment of \$12.00, and a simple meal for a few witnesses require little expenditure or organization, especially when compared with the elaborateness of a girl's first wedding with full traditional ceremony.

### Conclusion

In the above discussion I have attempted to explain the greater durability of marriage in one district of Negri Sembilan, as compared with Malay villages in Kelantan and Singapore, by the unilineal kinship organization of the former area.

This kinship organization provides both the incentives and the ability for members of the kin group to restrain a young kinsman from divorce during the adjustment period that immediately follows a marriage. These pressures lessen as the individual acquires the independent status of full adulthood, but at the same time the stability of the marriage comes to depend increasingly on the complex of practical interests that develops within it.

There is one serious difficulty facing this argument. The total figures for Negri Sembilan do not differ very much from those for the rest of the country, yet the matrilineal system of *adat perpatih* is generally held to be typical of the state as a whole.

Some help for the thesis may be derived from the inclusion within the boundaries of the modern state of Negri Sembilan of areas to which the *adat* is not indigenous, and also from the presence of migrants from Indonesia and other areas of Malaya within districts where the *adat* is nominally followed.

More probable is that, owing to the isolated and 'backward' character of the District where I was working, the traditional kinship system is better preserved. If this be so then divorce statistics, if available for smaller territorial units, such as the *mukim*, might serve as an index of social change. Their use in this way would not, however, be possible in the bilateral areas where, I suggest, a high rate of divorce is to be regarded as 'normal.'

Whatever the ultimate conclusion on divorce in Negri Sembilan as a whole, if the above discussion be regarded as a comparison of three specific communities the argument would not be seriously affected by contradictory information from elsewhere within the State, if only because I have observed people behaving in the way briefly described above.

### Notes

<sup>1</sup> The preparation of this note has benefited a great deal from the discussion of a previous draft by Professor Firth and the members of his Seminar in Anthropological Theory at the London School of Economics.

<sup>2</sup> J. Djamour, 'The Malay Family in Singapore,' thesis deposited at the University of London, p. 263.

<sup>3</sup> Rosemary Firth, *Housekeeping among Malay Peasants*, L.S.E. Monographs in Social Anthropology, No. 7.

<sup>4</sup> Djamour, *op. cit.*

<sup>5</sup> As the holder of a Treasury Studentship I was able to carry out fieldwork in Negri Sembilan between September, 1954, and February, 1957.



<sup>6</sup> Firth, *op. cit.*, p. 24: 'It is not possible to state categorically that to have been married three times is the most frequent condition. But it is legitimate to state that only a small proportion of people have had only one spouse, that a large proportion have had two or three or four, and that a fairly small proportion have had more than four spouses.'

<sup>7</sup> *Ibid.*, p. 28: 'It often happens that after the break-up of the initial marriage, the woman then being free to choose a man of her own liking, she will settle down to a long married life.'

<sup>8</sup> *Tempat semanda* is the collective name for the whole group of wives' kin as opposed to the *orang semanda* the 'outsiders' married to the women of the group.

## SHORTER NOTES

### A Gordon Childe Memorial

**209** The Hon. Editor of MAN (who hopes before long to publish obituary tributes to Professor Gordon Childe, F.B.A.) has received the appended communication from Professor W. F. Grimes, Professor Childe's successor as Director of the Institute of Archaeology, and commends it most warmly to all Fellows of the Royal Anthropological Institute and other readers of MAN who have admired the brilliance of Childe's work or felt the loss of his remarkable and much beloved personality.

By the tragic death of Gordon Childe, British and international scholarship alike have suffered a serious and premature loss. It is not necessary to re-state his contribution to the study of the past, his unique place in the development of archaeological studies, the range of his interests, his devotion as a teacher. Others besides archaeologists have derived benefit from Childe's thought; and not the least of his achievements was the sense of the essential unity in Man's past that his writings conveyed.

A number of suggestions have been made that there should be some form of memorial to Professor Childe, and it is thought that many people who knew him would wish to help bring this about. The Committee of Management of the Institute of Archaeology has therefore decided to invite contributions to a memorial fund. Decisions on the exact form that the memorial should take have been left until the support forthcoming for it is more certainly known; at the present time the suggestions are that a memorial lecture might be instituted, or a fund established to provide grants in aid for foreign travel. It is thought that either proposal would have commended itself to Professor Childe, for with a sufficient endowment the Institute would be able among other things to maintain and strengthen just those links with foreign scholarships which he always had so much at heart. The final decision in this matter will in due course be communicated to subscribers; in the meantime, donations should be sent as soon as possible to: The Secretary, University of London Institute of Archaeology, 31-34 Gordon Square, London, W.C.1.

**Tents and Domes in Persia.** By G. R. H. Wright, *Oriental Institute, Teheran—Bagdad. With a text figure*

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... it was the tent dwellers of Central Asia with their traditions of a round and domelike cosmic tent who account for the widespread popularity of the domical shape. These nomads of Asia had always lived, as many of them do today, in domelike Kabilka tents like those described by Marco Polo, Clavijo and other mediaeval travellers (E. Baldwin Smith, *The Dome*, p. 81).

It was, I think, Lethaby who first pointed out that the dome was characteristically a structure of rib and revetment, and hence, by inference, that our tendency to regard as basic the now familiar masonry form is a source of confusion in fundamental consideration of the feature. Confusion there certainly has been, for in the voluminous and acrimonious literature dealing with the dome and its origins, the most incongruous erections are set side by side. That all domed buildings, of whatever construction and sophistication, should be considered as forming part of a single stream of

development for which a common origin can be sought is something of an absurdity. This obvious statement may perhaps be excused as a necessary qualification of the following minor remarks.

The English reader when confronted with questions relating to the origins of domes in the East immediately adverts to Strzygowski. Unfortunately Strzygowski, as translated into English, is as perfunctory as he is definite on this matter. He states that the dome form is native to the nomads of the Altai-Iran region and that its natural expression is in wood. The idea of wooden construction is doubtless expounded in various German works, but in English it occurs *simpliciter* and is by no means self-explanatory. Indeed, it was only a visit to the area concerned which served to intimate to me that Strzygowski was referring to flexed wooden ribs and thus associating the origins of the dome in this area with the tents of the nomads—enlightenment in the first instance being afforded by a view of the framework of such a tent, a Turkoman *yurt*, set up in the ethnographical museum at Teheran.

The influence of tent forms on architecture, or their mutual interrelations, is by no means a novel idea; but, as it has not received great attention in English works, a brief note may not be otiose. Moreover, the Persian scene brings the matter sharply into focus, for in Persia the repeated invasions of nomads give the tent and tent form a lasting importance and influence.

The characteristic Central Asian nomad tent or *yurt* is convincingly described by Friar William of Rubruck in his account of his journey to the court of Mangu Khan during the years 1253-55 on behalf of St. Louis, the French crusading King. He speaks of it as follows:

Their houses in which they sleep, they base upon a round frame of wickers interlaced compactly: the roof consists of wickers meeting above into one little roundel out of which ascends a neck like a chimney [for ventilation]; they cover it with white felt and quite often they also coat the felt with lime or white clay to make it gleaming white, and sometimes they make it black. The felt round the neck at the top, they decorate with lovely and varied paintings.

Thus is pictured an erection which is quasi-architectural, a sort of primitive 'pre-fab,' and which has several features strongly reminiscent of a simple Eastern domed building. It is something more substantial than the normal idea of a tent, for, instead of being mere fabric pegged down, the side walls of the *yurt* are reinforced with collapsible lozenge lattices of laths which are held open by attaching them to uprights, while at the top they are affixed to the roof ribs. Of the features which raise the presumption of inference on other forms of building the most striking is the contour of the roof; for with their flexible ribs they attained a true dome with the compound curved contour typical of Persian domes. The lower set of ribs is allowed to curve out and then is drawn back in to meet the vertical wall giving the bulbous outline at the haunches which would seem to be foreign to any type of masonry construction. However, so proper is this curve to a flexed wooden rib, that (it is interesting to note) a tracing of the contour of a typical *yurt* roof extended below by its inverse image



gives an exact representation of a drawn Tartar bow. The upper section of the roof is framed with a different set of ribs and covered with a separate circlet of material that can be removed, or turned back to provide ventilation. This is suggestive of the wind cowl or small lantern which appears on some humble domed buildings. Finally, there is the similarity between the richly patterned textiles with which the *yurt* is sometimes covered and the glazed tile revetment now the characteristic decorations of domes in Persia.

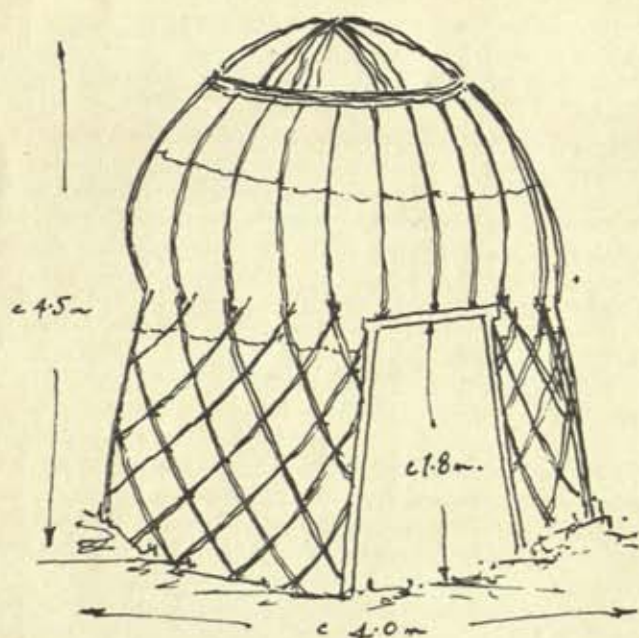


FIG. 1. SKETCH DIAGRAM OF FRAMEWORK OF 'YURT' IN TEHRAN ETHNOLOGICAL MUSEUM

Diameter at base c. 4 metres; maximum height c. 4.5 metres. The wooden ribs which frame the structure are c. 2.3 x 2 centimetres in cross section, secured (e.g. at the intersections of the wall lozenges) by leather studs and supplemented by lashings. The door frame is of substantial timber. The radial roof ribs are enclosed near the summit by a horizontal circle across which three groups of wickers are laid diametrically. For a photograph of such a yurt inhabited see A. C. Edwards, *The Persian Carpet*, London, 1953, p. 156, fig. 154.

Many Persian miniature paintings give excellent representation of richly decorated *yurt*. An illumination of the Tabriz school showing Majnum brought in chains to Leila's tent (see Pope, *Survey of Persian Art*, Vol. VII, Plate 910) renders the ornament of the roof of the *yurt* exactly as a glazed tile pattern, and the effect of the roundel at the summit is similar to the finial motif frequently found on domes.

It is manifest, of course, that all historical reference to *yurt* or pavilions of any description can only show interrelations and possibilities of original influence on architecture, for at this developed stage architectural forms are as likely to influence the pavilion as *vice versa*. Indeed, in the final analysis it is difficult to think from which side the original influence can spring, for a primitive withy hut may stand at the back of everything. However, perhaps sufficient has been remarked to show the possibility of a basic and pervading influence on Persian dome-construction by the *yurt* tents of the nomads of the Altai-Iran area (see Pope, *op. cit.*, Vol. II, *Tents and Pavilions*, *passim* for general background).

As a postscript it may be noted that the influence of tent forms on individual monuments or classes of monuments is patent;

notably the tomb towers surmounted with a cone which occur in Iran and adjacent areas from about the tenth century onwards. In this connexion an intriguing conclusion may be provided by reference to the influence which certain specific Persian tents might be supposed to have exercised on architecture of another day and age in a totally different context. In discussing the Persian spoils which remained in Athens after the defeat of the armies of the Great King, American scholars have suggested that the famous tent of Xerxes, a sumptuous pavilion, was most probably used by the Greeks to serve as the stage buildings and dressing rooms of the theatre in Athens, giving its name σκηνή, scene, to the later stone copies. Further I venture to suggest that the pointed roof of another Athenian building, the 'Tholos' might also have been derived from a Persian tent. It was nicknamed the σκιάς, shade, because it was built like a round parasol. The captured tent may have been first of all used by the town officials as their dining room and when the council later decided to build more solid accommodation for this purpose the old form may have been retained (see D. B. Thompson, 'The Persian Spoils in Athens,' *The Aegean and the Near East*, New York, 1956, *passim*).

#### A Witch Post from Scarborough. By Thomas Davidson. With a text figure

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Until late into the nineteenth century a great deal of the rural economy and hazards of animal husbandry were bound up in the belief in witchcraft. The loss of milk yield, and the mysteries of successful butter and cheese



FIG. 1. WITCH POST FROM SCARBOROUGH, 1870  
Specimen in the Pitt Rivers Museum, Oxford; height, 4 feet 11 inches

production were thought to be under the malevolent influence of witches, and in the absence of all but the most elementary knowledge of veterinary science all manner of superstitious practices were adopted as a means of protection.

The witch post shown in fig. 1 is such a device. It formed part



of the framework of the house usually fixed under the timber cross beams of the structure. Although the beams were invariably made of oak, the post was always made from mountain ash (rowan tree) and was marked by an inscribed cross. Its purpose was threefold, to prevent witches entering the house, in which case it was sited inside the doorway, to protect the house when it was positioned at the hearth, or as a repository.

According to a Yorkshire account, when the butter would not turn 'you took a knitting needle which was kept for the purpose in a groove at the top, and with it got out the crooked sixpence and

put it in the churn.' This neutralized the witchcraft—hence the witch's complaint:

Oh master, oh master, we can't do no good  
She's got a witch cross made o' mountain ash wood.

The witch post illustrated was part of a hearth seat of an old house in Scarborough. It is carved with representations of a cross, a heart stuck full with pins and different phases of the moon.

An interesting, and so far unexplained feature of these witch posts is that they are concentrated almost entirely in upper Eskdale.

## CORRESPONDENCE

'Insight: A Study of Human Understanding.' Cf. MAN, 1958,

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212 SIR.—The review of Bernard Lonergan's book *Insight* by Dr. MacBeath is disquieting. I think that the university which was mine by birth was also Dr. MacBeath's by adoption of sons. He will understand me, then, when I say that the imputation of *mala fides* to Lonergan is more redolent of the City of Belfast than of its University. I could (with as little reason, no doubt) discern the voice of the manse rather than of the academy in his review. I prefer to hear in it an echo of the Scottish philosophical anthem *Hume, sweet Hume!* It is not widely enough understood that no Catholic, even though he be a priest, is compelled to adhere to any particular philosophy, still less to defend one. The vogue of Thomism in certain circles today is as much a product of nineteenth- and twentieth-century thinking as is that of the theory of evolution in biology or the use of matrix algebra in physics, and for the same fundamental reason—because it appears to the *cognoscenti* to fit all the facts better.

The Thomistic 'coordinates' for the conceptual ordering of our thoughts upon the themes of being and changing cannot appeal to those who do not know them. They do not necessarily command the assent of those who do. This does not invalidate them; after all, some still regard even the Special Theory of Relativity as a needless complication, if not indeed a mathematical trick. Speaking as a natural scientist who has worked in more fields than one and who is not unread in the major systems of European philosophy, I find *Insight* to be as sincere a critique of epistemology as were those of Berkeley and Kant in their day. The fact that its author finds a positive and not a negative solution to his question is, perhaps, unfashionable in the Empiric Isle: it is no ground for asserting that he did not honestly follow the argument where it led him.

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'Enthusiasm and Restraint in the Study of Human Evolution.'

Cf. MAN, 1958, 189

213 SIR.—It appears from Dr. Ashton's article on my book *Man's Journey through Time* that he thinks that I have 'rushed in where angels fear to tread.' I fully realize the dangers of bringing the results of another discipline to bear on the professional anthropologists' views on evolution.

The graphical treatment in the book is merely the result of combining known measurements of osteometry with data provided by geochronology. How this can be done by very simple mathematical formulae is one of the theses of the book. The advantages of graphs showing temporal changes in man's skeleton and in his culture are obvious to those who can read them.

Dr. Ashton's objection to my graphical treatment arises mainly because of his own misconception concerning the factual experimental osteometric data and the purely theoretical interpretations and extrapolations of the graphs. It is perfectly justifiable to deduce some possible genetic relationship when measurements with recognized taxonomic significance lead to linear graphs as is the case, for example, with the Pithecanthropoids. Conversely no genetic connexion can exist between hominid types whose skeletal measure-

ments do not conform to a temporal sequence. This was clearly indicated by the graphical distribution of the points for the Australopithecinae relative to those of the other members of the Hominidae.

There may be differences of opinion as to their interpretation and I am pleased to see other points of view as to the use and significance of such graphs. But the main conclusions to be derived from them on the whole support the opinions of many leading anthropologists. The simple law of the physical evolution of the Hominidae based on the average gradients of the physical development graphs leads to a value slightly greater than one darwin. This is in close accord with Weidenreich's calculations and with the exponential equation of Haldane.

Similarly the general form of the cultural development graphs is a striking confirmation of the conclusions reached by Gordon Childe and L. A. White. More detailed results follow from a log./log. plot of these cultural data. These results, it is understood, will be published in the second of two articles which will appear in MAN shortly.

I must and do agree with some of Dr. Ashton's criticisms, where they are based on other opinions as to the mechanism of hominid evolution. I also agree with his reference to minor errors in the book which, it is hoped, will be corrected in due course. I do not, however, agree to some of his more personal references to amateurs and professionals which seem to be as unfortunate as they are irrelevant. Wells Museum, Wells, Somerset

L. S. PALMER

African Tone Riddles. Cf. MAN, 1956, 78

214 SIR.—Mr. D. C. Simmons asks for further information on the existence of tone riddles in Africa. They occur among the Luvala and allied tribes in north-west Northern Rhodesia. Thus in Luvala:

- Q. *ové mýyónu ungutátho*  
you who are going ahead, wait for me.
- R. *kutápángíla chámalóva*  
you are floundering in the mud (i.e. can't catch up).
- Q. *lishima lyátó lyásókisóki*  
a well full of fallen grass seeds
- R. *limbo lyátó lyámilónka*  
a village full of disputes.
- Or again in Mbunda:
- Q. *ou njambá mádi*  
here is a fat elephant
- R. *ou muvúlyá kwíta*  
here is a *Diplorrhynchus* bush exuding latex.

In one pattern there is a stereotyped phrase introducing the question, which lies outside the balancing tonal response, e.g.:

- Q. *muwande wanoko muwatwama kamwe / kéchi kumusámbililánga*  
in your mother's garden there is / which always greets her something

R. *mulémbelémbe wálsángu*  
leaves of bulrush millet.



Q. *muwande wanoko muwawama kamve / kákusésá mázó*  
in your mother's garden there is / with filed teeth  
something

R. *chipítá chámuvángwa*  
A Paropsia bush (because  
of toothed leaves).

These tone riddles are known in Luvala as *tushimutwiji*, a term applied to all riddles; they are used as a means of passing the time, especially among young people of both sexes; these will visit each others' village in parties to try out their skill against each other in this field. I know of no evidence to show that tonal or other riddles are used among the Luvala with specially erotic or slanderous connotations.

There is however another type of oral formulae which does fulfil Simmons's requirements for *double entendre*, slander, etc. These are

aphorisms consisting of two balanced clauses which often exhibit tonal correspondences, e.g.:

*mukanda kawéshi nawálakó*  
a circumcision ceremony without beer  
*chilolo késhi namangánakó*  
a headman without any sense.

In aphorisms of this sort, it is only necessary to utter the first clause to make the necessary imputation. These however are not riddles, and are known as *vishimo* (proverb, aphorism). The examples quoted by Simmons appear to resemble these aphorisms with balanced tonal correspondences, rather than the riddles which I quoted above. However, perhaps at this limited stage of data on this type of phenomenon in African oral literature, it may suffice to point out that tones are evidently used in more than one type of stereotyped utterance.

Secretariat, Lusaka, Northern Rhodesia

C. M. N. WHITE

## REVIEWS

### AFRICA

**The Prehistory of Africa.** By H. Alimen. London (Hutchinson Scientific and Technical), 1957. Pp. 438. Price £3 3s.

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When I purchased a French edition of this work a few months ago I hardly expected to see an English version on the market. That it should be profitable to translate and publish textbooks of this calibre and price as well as the popular works of interpretation and 'Romance of Archaeology' books, is a tribute to those who have done so much to educate the public in recent years. The English edition has a hard cover, a number of new illustrations, superior paper and printing, and is published at the same price as the French edition, or slightly less. These facts speak for themselves.

On the whole the book leaves a favourable impression. Professor Alimen has made a workmanlike summary of a vast and disorganized field of study. In France she is well known for her geological and pedological approach, as exemplified in the Introduction to her earlier textbook *Atlas de Préhistoire*, and in the present work she has made good use of the stratigraphic and climatic evidence which holds together African prehistory, and indeed often forms the only important or intelligible result of archaeological fieldwork in this continent. Happily neither the geology nor the human and animal palaeontology are here allowed to overwhelm the archaeological text.

*The Prehistory of Africa* is primarily a list of the differing stone industries in Africa during the period of the Old Stone Age. It is true that more modern periods are considered, in so far as their stone tools are concerned, and in the case of Egypt a more complete analysis is undertaken, but the emphasis is on the industrial succession of the stone industries in the different regions during the Palaeolithic. This bias reflects a weakness in African studies rather than any individual aberration of the author. Even so we may say that the evidence for the later periods, though treated with respect, does not always meet with understanding. The caption to Plate V, for example, 'Neolithic pottery from the Maghreb with various types of decoration' seems out of place in a book where the minutiae of technique of manufacture of the stone tools are treated in such detail. It reminds me of a label, in a museum where I once worked, which said: 'Dressed stones from various depths.' Egyptian pre-dynastic pottery receives similarly cavalier treatment. Miss Baumgartel's work on the cultures of prehistoric Egypt (1949) is not even mentioned, though her division between Naqada I and Naqada II is used without explanation in the text. A short classification of pottery by Petrie (1921) is repeated, and for the rest Professor Alimen would seem to rely on the French translation of Gordon Childe's *New Light on the More Ancient East*, itself a compilation.

This weakness is balanced by a really first-class chapter on the rock engravings and paintings of the Sahara and Southern Africa, containing a good deal of new material, and well illustrated with

drawings and photographs. The treatment of the palaeolithic sequence itself is encyclopaedic and often exemplary, though occasionally disappointing. It is sad to see that Tripolitania and Cyrenaica (Chapter II) are given only three pages of text. McBurney's detailed study of new sites in the area (C. B. M. McBurney and R. W. Hey, *Prehistory and Pleistocene Geology in Cyrenaican Libya*, Cambridge, 1955), and his earlier papers in the *Proceedings of the Prehistoric Society* (1948) and *L'Anthropologie* (1950) are ignored. Thus the important Levallois-Mousterian site of Sidi El Haji Creiem, near Derna, entirely escapes mention, and details of the three other principal sites in the area are lacking.

In general the bibliographies at the end of the various chapters appear to have been compiled in 1954, which is a serious drawback in a book published at the latter end of 1957. Also a number of curious mistakes appear under works referred to in the English language. Thus Dr. Addison's work is *Jebel Moya*, not *Jebel Maya* (p. 181); the capital of the Sudan is usually referred to in English as Khartoum, not Khartum (pp. 75-9), especially in the title of Mr. Arkell's publication (p. 181); and that distinguished anthropologist, until recently Hon. Editor of the *Journal of the Royal Anthropological Institute*, is not usually referred to as Dr. J. G. Trevoir (p. 75).

Still, there is no doubt that *The Prehistory of Africa* is, and will remain, an extremely useful textbook. The large number of illustrations, and in particular the regional maps and geological diagrams, alone make it indispensable.

G. DE G. SIEVEKING

**Third Pan-African Congress on Prehistory, Livingstone, 1955.**

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Edited by J. Desmond Clark. London (Chatto & Windus), 1957. Pp. xxxvii, 440, 81 plates, 7 colour plates, 122 text figs. Price £3 15s.

In 1947 the first Pan-African Congress on Prehistory met at Nairobi, the second was held in Algiers in 1952 and the third at Livingstone in 1955. The volume under review contains the proceedings of the third congress, some of the public lectures and brief reports of the excursions in the Rhodesias and Katanga. The 60 papers published represent a convenient report of the latest developments in the fields of African Quaternary Geology, General Palaeontology, Climatology, Human Palaeontology and Prehistoric Archaeology during the interval since the second congress in 1952.

The production is excellent and the editor, Dr. Desmond Clark, and his hard-working assistant, Mrs. Sonia Cole, are to be heartily congratulated not only on the quality and clear arrangement of the papers and reports but also on the expeditious manner in which they assembled the material and prepared it for the press. The proceedings of the first congress were not published until four years after the papers were read and those of the second took three years before they were printed. Many research workers were beginning to feel



that, owing to this delay, publication in the proceedings was not worthwhile. The present volume was available almost exactly two years after the congress itself and this is a considerable achievement when one considers the difficulties involved in the production of such a work.

Under the heading of Quaternary Geology, General Palaeontology and Climatology the papers fall mainly into four groups, dealing with (a) the Quaternary climatic sequences in various parts of Africa, (b) the physical and ecological features of the sites from which early human remains have been recovered, (c) a symposium on the Kalahari Sands, and (d) the dating of the Australopithecinae. The Human Palaeontology section includes further papers on the Australopithecinae, in addition to contributions on other African prehistoric human types. Dr. Cabot Briggs and Dr. Grobbelaar deal with the prehistoric origins of certain living tribes in the Sahara and with the origin and distribution of the Korana respectively.

In Africa, probably more markedly than elsewhere, the study of prehistoric archaeology is closely linked with that of proto-history and even of recent material cultures. Consequently, as would be expected, the section on Prehistoric Archaeology includes a wide variety of subjects, ranging from the somewhat controversial early pebble cultures to Iron Age cultures in Tanganyika, Uganda and Rhodesia. Six papers deal with the rock paintings of central and southern Africa and these are made all the more valuable by the inclusion of seven coloured plates, rendered possible by the generosity of Mr. A. L. Wilkie in bearing the full cost of the blocks.

The volume is completed by the inclusion of three public lectures given during the congress. These are by Dr. Desmond Clark and Roger Summers, reviewing archaeological developments in the Rhodesias, and by Miles Burkitt, on the place of archaeology in education.

In recent years reviewers have frequently complained that the high cost of certain publications places them beyond the reach of the private student. This volume is very moderately priced, thanks to a substantial grant from the Wenner-Gren Foundation of New York, and is indispensable to all who are interested in the wide aspects of African Quaternary Geology and Climatology, Human Palaeontology, Prehistoric and Protohistoric Archaeology, and Anthropology, which it covers.

JAMES WALTON

**Les Berberes.** By G. H. Bousquet. Editions 'Que sais-je?' Paris (P.U.F.), 1957. Pp. 120. About 350 frs.

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Berberology—the author requests our leave to use the term—is a predominantly French science, as Professor Bousquet reminds us at the beginning. He ends by agreeing with a judgment of an earlier scholar that Barbary is the most backward corner of the Mediterranean world and the land of the white Barbarians. This is true in the sense that Berbers are a Mediterranean people who have preserved a vigorous and archaic tribal organization right into the twentieth century: the last white tribesmen of the west.

Few men can be as well qualified as Professor Bousquet to introduce us to this predominantly French and otherwise little-known subject: a general expert on Islam who at the same time does detailed first-hand field work with an acute eye for the divergences between reality and books (be they books by or about Muslims), a jurist who is at the same time a sociologist and an author of a book on Pareto, a man steeped in his subject and an incisive, ironical, witty writer—he combines qualities which seldom mix. This excellent little book does indeed fully achieve what it sets out to do, namely, to introduce the reader to the main issues, achievements and background of Berber studies. Jointly with R. Montagne's *Vie Sociale et Politique des Berberes*, to which he refers, and indeed with his own *L'Islam Maghrebin*, this book does provide the best and most concise initiation to the subject.

If there is a general point about which I should like to record a doubt, it is whether Professor Bousquet and French Berberology in general have not borrowed the wrong things from Ibn Khaldun (and, perhaps, failed to follow up some valuable ones). I have in mind the speculations of Gautier and Marcy, which Bousquet quotes, with which he agrees in part and which he appears to consider a sound type of enquiry, which interpret Berber history as

a kind of long series of matches, return matches, etc., between the big 'genealogical' groupings found in Ibn Khaldun. I doubt whether interpreting historical events as a tug of war between Branes and Bott, or a triangular fight between Sanhajja, Masmouda and Zenata, throws any light even on the Middle Ages, let alone anything since. These conflicts, even if they occurred, cannot be seen as literal, conscious conflicts of a kind of nationalism; the general lack of a supra-local Berber consciousness, which the author stresses, also excludes a conscious sense of belonging to these nearly as large, and extremely dispersed, groups. Nor can it be made plausible by insisting on the ecological differences between the groupings, and saying that the conflict appeared as one between nomads and sedentaries, etc.: that is excluded by lack of correlation between ecology and this type of classification, and by the frequent transformations and migrations. Neither objectively nor subjectively, then, do those groupings have a convincing reality. Bousquet points out, as others have, that Berbers have a genealogical (rather than geographical) sense of belonging: they do indeed, but it seems to me that, through Ibn Khaldun, they have transmitted such genealogical pseudo-conceptualizations to French scholars. The valuable parts of Ibn Khaldun are perhaps rather the clues (some of which Bousquet mentions but does not pursue), which he provides to the political failures of the Maghreb prior to modern times: it is, I suspect, in the political sociology of tribal-urban relations, rather than in some alleged psychological idiosyncrasies of the Berbers, that the explanation of those failures is to be sought.

However, these tentative doubts, even should they turn out valid, relevant as they are to but a very small part of its contents, in no way undermine the value of this excellent little introduction.

ERNEST GELLNER

**Ethnographic Survey of Africa: The Wolof of Senegambia.**

By David P. Gamble. London (Internat. Afr. Inst.), 1957. Pp. 110. Price 16s.

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The arrangement of the material in this book follows the established pattern of the Survey and it provides a comprehensive summary of existing ethnographical data on the Wolof, who comprise a number of related groups living on the Western coast of Africa between the Rivers Senegal and Gambia. There are also short notes on two neighbouring groups, the Lebu and the Serer.

Three facts have made Mr. Gamble's task a hard one. In the first place, as the Wolof do not form one homogeneous tribe, there are many differences from one group to another. At the same time, in common with many other areas in Africa today, changing social conditions have led to the emergence of new cultural patterns. The wide variations in behaviour and belief make it difficult to generalize. A third difficulty arises from the nature of the material available. Apart from Mr. Gamble's own fieldwork data, there was relatively little modern material at his disposal. Much information is derived from brief references in the general literature relating to Senegambia. The result of his labours is a fair appraisal of present-day knowledge of the Wolof. In particular I should like to commend the extremely comprehensive and well annotated bibliographies accompanying the text.

MARY BIRD

**African Negro Sculpture.** The University Museum Bulletin, Vol. XXI, No. 4, December, 1957. University Museum, University of Pennsylvania, Philadelphia

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Written for the novice, this Bulletin is welcome to the student, for it illustrates many carvings from the admirable collection of the University Museum. Some of the pieces have not been published and others are to be found only in scattered reports and catalogues.

The Introduction by Dr. Carleton Coon indicates how much is still to be discovered of the origins of the Negro and his art. It is, however, a bit startling to read that the mouth of the Niger was without doubt the centre of dispersion of the Negro, and that the anti-malarial effects of sickle-cell anaemia can be accepted without question.

In the major portion of the Bulletin ('A Walk through the Gallery'), Mrs. Margaret Plass acts as docent, drawing attention to about



one hundred carvings now on exhibit in the new African Gallery. It is not an exhaustive catalogue; scholars are invited to use the Museum's fully documented formal catalogues. Rather, the author makes use of a docent's prerogative to point to particular pieces, draw comparisons, state facts and make judgments. It is, in effect, an introductory lecture. The facts are sound, the comparisons apt and the judgments informed.

From the point of view of art criticism two of the points raised are particularly well taken. Admiration for the exotic all too easily leads to an uncritical admiration of every adze cut as the stroke of genius; yet sensible, and sensitive, observation must yield to the conclusion that individual works of African art, like those of any other culture or period, can be superb, mediocre or downright incompetent. Thus, when the author describes a Benin bronze (p. 45) as decadent (when it is) and notes the lack of originality in many Baule pieces (p. 17) she is quite rightly setting the reader on the path to connoisseurship. Further, she notes that the easy resemblances between Modern and primitive works are essentially superficial. She might have added that it is often the Modern artist who gains by the comparison.

The map, though excellently drawn, fails, with the exception of the Ashanti, to locate the tribes. Thus, although nine tribes are listed for the Belgian Congo, they might be lost anywhere in its reaches.

Despite the introductory nature of the Bulletin, reference to the collector and the dates of collection and acquisition would have been a great aid to the scholar, and might have impressed the novice with the need for precise data.

For the most part the photographs, by Reuben Goldberg, are clear and refreshingly devoid of that obscurantist darkness called artistic lighting.

ROY SIEBER

**The Egba and their Neighbours, 1842-1872.** By S. O. Biobaku. Oxford (Clarendon P.), (London: O.U.P.), 1957. Pp. 128, 4 maps. Price £1 15s.

The Egba are one of the most historically interesting of the Yoruba tribes, as the first to feel the full impact of European penetration, the first to react against it, and the last to surrender the forms of national independence to the government of British Nigeria.

Dr. Biobaku devotes the first two chapters of his cautious and unpretentious narrative to the origins and character of the autonomous communities of the Egba forest, their dispersal under pressure from Ife and Ijebu after the breakdown of the Oyo hegemony, and the early days of the fortified settlement of Abeokuta where the scattered populations of some 200 of the old Egba towns gathered together for mutual protection under the leadership of Shodeke about 1830.

The reader who expects too much from oral tradition may well be rather disappointed with this section, for Dr. Biobaku is not the man to be driven beyond his evidence, but it rectifies a number of errors in the hitherto accepted account and provides a useful corrective, from the Egba point of view, to the Oyo version of events popularized by Samuel Johnson's well-known *History of the Yorubas*.

Abeokuta was in reality not so much a city as an armed nation in camp, a congeries of small settlements, each of which retained the name, the memory and the municipal institutions of the old town from which its inhabitants had come. Inevitably the war captains (*Ologun*) dominated the political scene, and after the death of Shodeke none of them was strong enough to impose his will upon the others. Attempts to devise a supreme civil authority, whether on the traditional pattern through a new All-Egba Ogboni society or on 'civilized' lines through the United Egba Board of Management, were frustrated by the particularist spirit of the several townships and the rivalry between traditional office-holders and the 'educated' Sierra-Leonians. Without effective government, the Egbas were unable to follow a consistent policy in their relations with rival Yoruba states and the British administration at Lagos, or to present a united front to the intrusive influences represented by missionaries and traders.

Dr. Biobaku's interpretation of the confused annals of the Yoruba wars makes very good sense, and if at times he seems to have taken

more trouble to understand the Egba side of events than the British, this is a useful corrective to the balance usually struck in more orthodox works on 'colonial' history.

D. H. JONES

**Townsmen in the Making: Kampala and its Suburbs.** By Aidan W. Southall and Peter C. W. Gutkind. *East African Studies No. 9.* Kampala (E. Afr. Inst. of Soc. Res.), 1956. Pp. xiv, 272, 3 maps. Price 5s.

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This large and well produced stencilled publication contains two reports prepared as part of a general survey of African life in Greater Kampala. One of the districts was chosen to represent the densest type of uncontrolled and primarily African settlement, the other as an intermediate situation illustrating the transition towards these conditions from a previously rural community. They are introduced by a short historical study of Kampala's growth, and rounded off with an incisive review of administrative problems.

Each of the reports contains sections devoted to land and housing, economic activity and marriage; they draw upon the survey results for basic social statistics but supplement them extensively with case histories, anecdotes, overheard conversations, etc., in a fashion that gives the flavour of life in such neighbourhoods as no previous African study has ever done. The sections on marriage are particularly valuable. Nevertheless, this remains a descriptive work with a highly intelligent sociological commentary. For the theoretical conclusions we must doubtless wait for the full account of the survey, while congratulating the Institute on giving us so attractive a foretaste at so cheap a price.

MICHAEL BANTON

**Shifting Cultivation in Africa.** By P. deSchlippe. London (Routledge & Kegan Paul), 1956. Pp. xxxi, 304, 96 plates, 4 charts. Price £2 2s.

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Mr. deSchlippe describes himself as a practical agronomist who has undertaken research on the borderline of agronomy and anthropology, first as a hobby and then professionally. Most of the material was collected while he served as Senior Research Officer of the Yambio Experimental Station in Zande District of the Sudan. He is working towards a solution of what he sees as Africa's most pressing problem: the improvement of the agriculture of the wet tropics to meet the increased demands for food in the developing industrial centres. As the first step, he claims to offer in this volume a pure description of the system of agriculture of the Azande of the south-western Sudan, while withholding interpretations and practical solutions for a second volume.

Mr. deSchlippe does not tell his readers that he had prepared a specific plan for the reform of Zande agriculture (see 'Hedge Strip Farming, A Plan for Zandeland,' *Sudan Not. and Rec.*, Vol. XXXVI, part 1, June, 1955, pp. 6-36) prior to publication of the book under review and that, furthermore, he had submitted a similar scheme for official consideration in 1951, even before the materials had been fully compiled. The book contains, in fact, a good deal of interpretation and evaluation interlarded with description, and involvement with a specific scheme of action has clearly influenced the premises and judgments of the author.

In his investigations Mr. deSchlippe used six or more Zande clerks as interviewers who visited a number of homesteads each day and noted down the single main activity of each adult. During the first year, 180,000 entries were recorded and subjected to statistical treatment. The author feels that the mass of data overcomes the problems of difficulty of observation of the whole agricultural cycle and the typicalness of activity, but one can hardly fail to be struck by the assumption that sheer quantity can overcome problems of reliability which the author discusses in a candid fashion. The notations of the intermediary observers were supplemented by occasional observations on the part of the author of the most interesting fields and by verbal information received from informants. Although he is critical in his introductory remarks of the value of verbal information, the author has obviously depended heavily upon such sources.

The admirable maps of Zande holdings through a period of years reveal that the author's labour has been expended on a study of Zande agriculture practised in an abnormal setting and not of the traditional system as he claims. These sketches are of the fixed,



arbitrary family plots after the mass resettlement of population initiated in 1946. Since the administration has interfered with their normal selection of fields, the Azande have over-cultivated the areas at hand. They often complained to me, during the period when I was conducting anthropological research among them, that they were being forced into abnormal agricultural practices. The author's distant vantage point allowed him to view the resettled scene as a normal one, with only minor exceptions to traditional practice. We may note also that his reform scheme for Zande agriculture is based on this type of resettlement and that his implicit condemnation of Zande practices can be used to justify the crop-rotational features of his plan. He has failed, however, in his intention of interpreting the agricultural system in terms of its environmental and traditional cultural limitations by not accounting for the direct and indirect limitations imposed by the administration.

A great handicap for the author is his definition of shifting cultivation, which, according to him results in the shifting of homesteads to follow the shifting of fields in the search for fertile land. This definition does not fit the Azande situation nor is it applicable to much of tropical Africa, where a similar form of agriculture is employed with wide variety of settlement patterns, including permanent villages. The lack of distinction between the pattern of shifts in cultivation and the pattern of shifts in residence contributes to the lack of conclusiveness of Mr. deSchlippe's findings. He states that his work will enable us to determine the cultural and the environmental limitations in the Zande system of agriculture, yet he obviously has made only very limited investigations into cultural factors in the movement of homesteads, and when he speaks of environmental limitations which lead to over-cultivation and movement of homesteads, he is often reporting the effects of the confusion caused by resettlement. He alludes to over-cultivation, bush fires and density of population as factors which contribute to increased distances between home and fields, but stops short of any attempt to find out what these factors mean in actuality, and is content to restate his premise as a conclusion, with the weight of the book implicitly behind it. When he concludes that the '... periodic dying of the homesteads is one of the most important features of shifting cultivations, and as a traditional limitation of general character it is the greatest obstacle in the way of Africa's progress,' he is obviously preparing his readers for his proposals for improving African agriculture.

The author's handling of data often misses the goal he sets for himself in the theoretical sections of the book. When his proclivities for drawing conclusions and making generalizations are

added to his broader tendencies, we often have difficulty in knowing what is based on observation and what is surmise. Consequently, although the book in its non-interpretative aspects offers the best available collection of information about Zande agriculture, caution must be recommended, as the validity of the data is rather uneven.

The work can be seen to be of importance as a means of conveying a vivid impression of Zande agriculture and it seems to deserve at least the author's evaluation of one of its sections—'significant, if not entirely accurate.' If one is not concerned with full accuracy, there is a great deal of value in this book and it adds considerably to the too few books available on the subject of tropical agriculture. From this point of view, one must regret that the author was not more successful in his intention of making it a purely descriptive account. One regrets, also, that involvement with a particular line of action prevented him from making fuller use of unusual experience and insight in his pioneer effort to develop a middle ground between agronomy and anthropology.

CONRAD C. REINING

**Les Esprits de la Vie à Madagascar.** By Jacques Faublée. Paris (P.U.F.), 1954. Pp. viii, 161, 15 maps and line drawings. Price 600 francs. **La Cohésion des Sociétés Bara.** By Jacques Faublée. Paris (P.U.F.), 1954. Pp. 143, 11 line drawings. Price 600 francs

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Dr. Faublée, who contemplates revisiting Madagascar shortly in order to apply his study of the Antaimoro manuscripts in Arabic characters to fieldwork among this important southern group, may now fairly be considered the most experienced of the younger workers in the Malagasy field, his interests being both wide and constantly true to the life and spirit of the Malagasy. His books convey to the reader an immediate and cogent picture of the peoples of the island and the reader may trust to the impressions that he receives about them from Dr. Faublée's writings as well as he may trust the facts given. Throughout both these books the facts are selected with care to express those central continuing truths which mould and support and give meaning to these small communities in all the vicissitudes of life (including those of their meeting with Western culture). Other works are soon to be published by this author, and a detailed study of Arabic influence foreshadowed in a contribution 'Antemuru' in the *Encyclopédie de l'Islam* promises an investigation and clear statement of the problems of this subject which will be of the greatest importance in the ethnographical study of geomancy in Madagascar.

The illustrations in both books are very well done.

MARY DANIELLI

## AMERICA

### The People of Puerto Rico: A Study in Social Anthropology.

By Julian H. Steward and others. Chicago (U. of Illinois P.), 1956. Pp. ix, 540. Price \$10

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This massive volume contains the principal results of a large-scale study of life in Puerto Rico. Five fieldworkers report separately upon their investigations, while the research staff under Steward's leadership are collectively responsible for an introductory analysis of the population's cultural background and for a concluding discussion of regional variation, nationality, and cultural change. Few comparable territories can have been studied so comprehensively and the book will doubtless become an indispensable work for those interested in the ethnography of the Caribbean, in the comparison of tropical peasant communities, or in the interaction of environment and society.

It is impossible here to do justice to the wealth of material contained in the field studies. The first, an account of the sub-cultures of a tobacco and mixed crops municipality, argues that the social structure flows more from the society's productive arrangements than *vice versa*. The second, an examination of a traditional coffee municipality, traces the effects upon the local culture and economy of changes in the island's external relations. The third, an analysis of the communal organization of workers on a government-owned sugar plantation contrasts with the fourth, which deals with a plantation proletariat among whom the processes of change have

progressed further. The author of the fourth study finds the influence of environmental and economic factors upon culture so definite as to suggest the existence of an 'international sugar cane plantation way of life.' The fifth contribution describes the prominent families of Puerto Rico and the growth of an élite of businessmen, administrators and professional people.

Professor Steward claims for the work three special virtues: (a) a clarification of the cultural method as anthropology's contribution to area studies, exemplified in the attempt to ascertain the factors and processes which have brought about the development in Puerto Rico over recent centuries of distinctive rural sub-cultures; (b) the study of local sub-cultures as a means of conceptualizing the totality of the cultural phenomenon in an area unit; (c) the elaboration of a method for stating cross-cultural regularities. The initial aim having been the descriptive integration of their materials, the authors' final hypotheses are framed as purely empirical propositions, thus: 'The family-owned and operated hacienda developed as an important rural sub-cultural type when the following conditions appeared: (1) an assured market for a cash crop; (2) adequate shipping facilities; (3) environmental potentialities for growing a cash crop; (4) sufficient credit to finance the necessary overhead; (5) assured profits.' This procedure may well lay the foundations for a useful typology of culture change, but until the ideas can be framed as analytic propositions (and I do not see how they can within this theoretical



framework) they will not lead to the demonstration of meaningful relations between the components of change.

In certain respects, then, the book seems to mark the end rather than the beginning of a trail. It states the best case that can be made for a theory based on communities conceived as socio-cultural units ('from a cultural or holistic point of view an area must be viewed as a well integrated socio-cultural system,' p. 26). But, as has been said, cultures are units only in so far as they are tied to bounded social structures. Many would go further than Professor Fortes in this and deny that societies are units of study or that they have boundaries. Tribal societies are homogeneous only relatively to other societies and, as Professor Gluckman has recently shown, it is often more illuminating to proceed from internal oppositions than from the postulate of integration. The concept of culture as a unit is no way out for it can be divided into sub-cultures and sub-sub-cultures indefinitely. One of this book's minor virtues is to make us reconsider the limitations of our Comtean and Durkheimian inheritance and to enquire whether action theories on the Weber-Parsons model offer any hope of better success with problems of culture change.

MICHAEL BANTON

### Burning Water: Thought and Religion in Ancient Mexico.

By Laurette Séjourné. London (Thames & Hudson), 1957. Pp. xiii, 192, 22 plates, 82 line drawings. Price £1 5s.

Laurette Séjourné is an archaeologist who has recently completed two seasons' excavation at Teotihuacán where she has made some important additions to our knowledge of that immense site of which only about a tenth has so far been uncovered. Previously, she was a student of Caso, having worked at the Instituto Nacional de Arqueología e Historia and then as assistant to Alberto Ruz in his work at Palenque in 1951.

The book is divided into four parts. In the first, the author seeks to establish from the chronicles the pre-Aztec religion in order to explain the duality of Aztec life at the time of the Conquest, with its apparent highly developed spirituality on the one hand and a completely ruthless attitude towards the individual through wholesale massacres on the other. In the second part, myths recorded in the historical and native texts are used to reconstruct the religious principles of the spiritual concepts of the Quetzalcoatl doctrine, which provides the Aztec view of the cosmos and man's position in relation to the divine principles governing it. 'Each of the Nahuatl gods is an actor whose role is rigorously determined by the dramatic needs of the whole' (p. 183). The gods, through their distinguishing ornaments and ritual, teach man how to reach, step by step, the higher levels of spiritual freedom. The gods are associated with the various revolutions of the planets which symbolize the process of transfiguration of matter. It is this process which man, with the example of Quetzalcoatl to follow, strives to achieve: the conquest of matter in order to obtain the spiritual whole only to be found in heaven. But only the nobles and chiefs can hope to attain this heaven; an earthly paradise is reserved for the commoners—an interesting reflection of the structure of Aztec society.

In the third part, the author tests the symbolic interpretations of the myths she has made against the meaning of the glyphs of the symbolic language of Nahuatl religion as manifest in archaeological sites from earliest times to most recent. The evidence for the study of actual Nahuatl symbolism rests with the fresco and *champ-levé* decorated pottery and the few murals that have been unearthed at Teotihuacán. In the fourth part, through examples of deities from sculpture and the Aztec and Mixtec codices, a close correspondence of the Teotihuacán murals with the myths recorded by the chroniclers is established.

Laurette Séjourné maintains that archaeological excavation confirms that Teotihuacán was the first centre and for long the only one where the cult of Quetzalcoatl existed. The plumed serpent did not exist before Teotihuacán; similar images from other sites are later in date. This, she says, proves that Teotihuacán was the first Nahuatl city. She thus returns to the earlier view of such as Seler that Teotihuacán was the capital of Quetzalcoatl and not Tula which is contrary to the present general consensus of opinion (this view is elaborated in the author's 'Tula, la Supuesta Capital de los Toltecas,' *Cuadernos Americanos*, Jan., 1954, pp. 153-169). She points out that

ten seasons' work by Jorge Acosta at Tula-Xicotlán has unearthed only a second-rate centre and no examples of painting for which the Toltecs were renowned.

This is a very interesting study of Nahuatl religious symbolism but it is disappointing that Séjourné's thesis suddenly stops in the middle and is not carried through to a logical conclusion; and sometimes she is more insistent than reasoned. After considering the symbolism in detail of five important gods (Quetzalcoatl, Xochipilli, Xipe Totec, Huitzilopochtli and Tezcatlipoca) she suddenly calls a halt and confesses 'to feeling incapable of describing the vision of the Nahuatl world which we have glimpsed in the course of our work,' just when her interpretations are beginning to provide a view of this complex religious system. It is hoped that a full and rounded study will appear one day.

The plates and line drawings are of a high order. Irene Nicholson is to be congratulated on the excellence of the translation for the author sometimes uses a somewhat mystical form of expression to convey certain images implicit in Nahuatl symbolism. These are hard to retain in translation.

PHILIP DARK

### The Trumai Indians of Central Brazil. By Robert F. Murphy and Buell Quain. New York (Augustin), 1955. Pp. xii, 108.

Price \$2.75

Not many anthropologists would welcome the task of writing up a dead man's material from his field notes. Most would probably shudder at the thought of having their field work written up posthumously by someone else. So it is remarkable, in a way, that the results of Buell Quain's work among the Trumai ever reached publication. They did so only because the Trumai are now virtually extinct as a functioning society, so that Quain's notes have acquired a unique value. Robert F. Murphy's monograph, based on these notes, provides a triumphant answer to the old conundrum which asks whether unsatisfactory data are better than no data at all. In this case, they definitely are.

In the first place, ethnographers will be grateful to Murphy and Quain because the region of the headwaters of the Xingú is one of the best-known and possibly one of the worst-studied in Brazil. Information such as Quain's, even after only four months in the field, is therefore a valuable contribution to the comparative ethnography of the area; especially since the Trumai are a linguistically isolated group and, in all probability, comparative newcomers to their habitat.

The book is, however, no mere ethnographical survey. It is a study of social disorganization, which is made all the more tantalizing because of its acknowledged incompleteness. The gaps in Quain's notes are reflected in Murphy's text, and, inevitably, many important questions remain unanswered. Why, for example, did the Trumai lay such stress on virility and hardness in men, when there is no mention of corresponding activities in which these characteristics could find their expression? There are only brief allusions to the warrior past of the Trumai, and not all of them need to be taken too seriously. It is not necessarily significant, for instance, that Karl von den Steinen was warned not to visit the Trumai at the end of the nineteenth century, for travellers in Central Brazil are even today 'warned off' tribes that are not particularly bellicose. On the other hand we are specifically told that 'among the Trumai, no special prestige was derived from warfare, and the men did not boast of their prowess as warriors' (p. 15). Why then should men be hard? If it was in order to gain prestige through wrestling, Quain does not stress this point; nor is it clear how this quality of 'hardness' fits in with Trumai ideals, for they are presented as a timorously aggressive and anxiety-ridden people. This vagueness about the acquisition of prestige in Trumai society is an important weakness in Quain's material. He never arrived at a satisfactory understanding of the institution of chieftainship, or of the alternative means of acquiring authority within the tribe, fundamental considerations in any analysis of social disruption.

In view of this disruption of Trumai society at the time of Quain's field work, it is not surprising that we do not get a clear idea of Trumai values, nor that the data on kinship and formal relationships within the village are so incomplete. Even so, it would have helped the reader to make some sense out of the puzzle if Murphy had



published some of Quain's genealogies, possibly in conjunction with household charts. Religion and mythology are also treated sketchily. As far as Quain was able to observe, the Trumai no longer attached any great importance to such matters, and he would not, in any case, have mastered enough of the language to carry out a detailed investigation. It is doubtful, perhaps, whether the remarkable lack of ceremony, which Quain noted among Trumai men on 'ceremonial' occasions, necessarily indicates, as he and Murphy assume, a lack of interest in the proceedings. Other research workers among the Indians of Central Brazil have been struck by the apparent casualness displayed on occasions when they would have thought some sort of tension more appropriate. An understanding of such behaviour demands an extremely thorough knowledge of the people and the ceremony concerned. In this case it is clear that the Trumai's 'loss of faith,' which is essentially what the monograph documents, made material on religion doubly difficult to collect.

The purpose of a monograph is to supply information in such a way that it is susceptible of analysis. The tragedy of Murphy and Quain's work is that not quite all the information is there, and

therefore any analysis is both difficult and hazardous. Murphy has, nevertheless, made the attempt, and, though one may wish to disagree with some of his conclusions, one cannot criticize the way in which he proceeds. Buell Quain must have been an unusually methodical and perceptive worker for such collation and analysis of his material even to be thinkable after so short a time in the field. To point out the gaps in his information is only to stress what he was conscious of himself, and what he had later hoped to rectify. As it is, the monograph gives a valuable insight into the decay of Trumai society and the vicious circle of depopulation, disorganization and demoralization in which it was caught up. Had Quain lived to complete his work, he might not only have extended his research among the Trumai but have developed his analysis of what Murphy calls the 'bounded social system' of the Upper Xingú basin. The tribes within this system, linguistically diverse but culturally similar, joined through ties of kinship, trading and war, offer a magnificent opportunity for the study of a whole network of social relationships on a scale similar to that of the 'Kula' ring. It is a great pity that he did not live to do it.

D. MAYBURY-LEWIS

## ASIA

**Prehistory and Religion in South-East Asia.** By H. G. Quaritch Wales. London (Quaritch), 1957. Pp. [viii], 180. Price £2

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In this volume Dr. Quaritch Wales continues the reconstruction of the early history of religions in South-East Asia which he began in *The Making of Greater India* (1951) and continued in *The Mountain of God* (1953). Starting from an assumed background of universal animism he traces different stages of religion through belief in a Lord of Beasts during the food-collecting economy to that in a sky god derived from Babylonia, who gives precedence to an earth god when emphasis is laid on agriculture, or to associated gods of earth and sky with a cult of ancestors and phallic symbolism. What he is particularly concerned with in this volume, however, is to demonstrate the penetration of a shamanic cult from central Asia with the spread in South-East Asia of the Dongsonian bronze culture, and to show how the practice of shamanism is to be distinguished from mediumistic possession by god or spirit, and how the difference and contrasts between the post-Indian cultures of Java and Cambodia, between the Chams and the Khmers, for instance, depend on the extent and nature of the resurgence of primitive religions in the decadence of Hinduism and Buddhism. Chapter III on the Bronze Age religion occupies fully a third of the whole volume, and is devoted to showing the connexion between the practice of shamanism and the bronze-age culture related in South-East Asia to Dongsonian art motifs and to what he calls the 'primary bronze drums' of Dongsonian associations. The earlier of these 'drums'—one would prefer the term 'gong' since they have no membrane—are associated with bronze-age finds in Tonkin, and they have been continuously manufactured ever since, or at any rate down to the end of the last century when the Karens seem still to have made them occasionally, much on the ancient pattern.

Dr. Quaritch Wales writes with much learning, with an intimate acquaintance with his area, and convincingly as one having authority on his subject. Incidentally his reconstruction of the successive phases affords an attractive explanation for some phenomena of secondary and tertiary disposals of the dead practised in Assam, sheds fresh light on the Javanese shadow play, and puts the quietus on the Horus emblem in Javan art. But there are one or two minor points not so satisfactorily dealt with. He finds no trace of shamanism among the Andamanese because 'they have been too deeply affected by Hinduisms'—by which their culture, one would have said, has been completely untouched, though they do have certain stories about the rainbow which are slightly suggestive of shamanism. On the other hand he omits reference to the adjoining Nicobarese, whose medicine men are apprenticed and trained in a way decidedly suggestive of that cult. He says, rightly no doubt, that shamanism is uncharacteristic of Assam tribes and that Dongsonian influences scarcely penetrated there. In material culture however they appear among Singphos and Kukis, in the religious field in the Ao tribe, probably, and perhaps in the were-leopard beliefs of several tribes.

One would have liked a specific reference to some authority for the statement (p. 37) that 'special efforts are made to transfer the soul force of the [Konyak Naga] chief to the tree after his death' for the practice is quite unfamiliar. Finally Dr. Wales really must not speak of 'the rite of the Palaeolithic hunters of Europe who, at the beginning of each season, did not eat the first slaughtered reindeer, but threw it into a mere, weighted with a stone, apparently as an offering to the spirit of the herd' (p. 14), as if this were an ascertained fact. Professor Gordon Childe did say so both in *What Happened in History*, as an 'interpretation,' and in *Prehistoric Migrations* as if it were a known truth, but it cannot conceivably be more than a hypothesis to explain certain finds.

The volume is well printed and contains some two dozen illustrations and a useful index.

J. H. HUTTON

**Afghanistan.** Edited by Donald N. Wilber. *Human Relations Area Files*. New Haven, 1956. Pp. 501. Price \$8.50.

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**Annotated Bibliography of Afghanistan.** Edited by Donald N. Wilber. *Human Relations Area Files*. New Haven, 1956. Pp. 120. Price \$5.50.

This monograph on Afghanistan is one of the 'Country Survey Series' produced by the Human Relations Area Files. The materials presented derive from the existing literature, direct from the field experiences of several contributors, and from a limited number of interviews with Afghan students in U.S.A. The monograph covers a series of standard topics such as History, Geography, Economy, and various fields of State Organization; it also contains chapters on purely anthropological topics such as Ethnic Groups, Social Organization, the Family, and Social Values.

The work is a compilation of draft chapters written by different authors. Unfortunately these have not been properly integrated and the book tends to be repetitive. It also bears the stamp of the card-index system on which it is based—for example, on p. 55 there is an item reporting that in 1924-27 pagan customs among the Kafirs were still 'in full bloom,' while after scattered comments on Chahar Aimak, Baluchis, Brahuis, etc., there is a further item on p. 57 reporting that in 1928 the Kafirs were 'proud recruits to Islam' and paganism was discredited.

As the editor points out, published material on Afghanistan is inadequate. Indeed, I wonder whether it is possible to write a book like this at the present stage. But if it is to be done at all, it requires great discipline and careful scholarship. Instead we are presented with a hash of unidentified information, often without reference to sources even where verbatim citations are given (e.g. pp. 45f.). Much of this information is surprising, or inexact, or even erroneous. We are thus told of Turks speaking a Scythian dialect (p. 50), that Baluchis came from Central Asia in the early centuries of the Christian era (p. 55), that the Chitral Kafirs have all been converted to Islam (p. 58); or in the section on Social Organization that *hamsayahi* clients are 'former slaves' (p. 323), that smiths and



weavers in Afghanistan 'correspond to the outcasts of India' (p. 327). The chapter on the Family contains a delightful theory of fecundity: 'Since the religion teaches them that it is meritorious to bring up souls to confess that God is one . . . each family has as many children as possible' (p. 340). Such generally prevalent weaknesses, together with the total inadequacy of the maps and the high price, very severely reduce the usefulness of the monograph as a source or reference book.

Of greater use is the companion volume, the *Annotated Bibliography of Afghanistan*. The remarks on some works bear evidence of rather superficial evaluation—compare, for example, the description of Robertson, our main source on Kafirs, as 'A British Army officer, interested in linguistics and ethnography, tells in narrative form . . . whereas Shakur's small pamphlet, based almost exclusively on Robertson, is described as 'A first-hand survey of part of the region by the Curator of the Peshawar Museum. . .'. However, the coverage is very wide, the discussions of the relative accuracy of sources in different fields is clear and convincing, and the organization of the sources under various headings seems convenient and useful. Filling a need only partly satisfied by Akram's *Bibliographie Analytique de l'Afghanistan*, the Bibliography should prove a valuable help to anthropologists interested in the Middle East and Central Asia, as well as to Afghan specialists.

FREDRIK BARTH

**Bulletin of the Tribal Research Institute, Chhindwara, No. 1.**

**229** Chhindwara, 1957. 42 leaves. **Bulletin of the Department of Anthropology, Bihar University: Studies in Bihar Tribes, I. Ranchi, 1957. Pp. 79. Study of Changes in Traditional Culture.** Edited by K. P. Chattopadhyay. Calcutta (U. of Calcutta), 1957. Pp. 139. **Contributions to Indian Sociology.** Edited by L. Dumont and D. F. Pocock. Paris and The Hague (Mouton) (U.K. agents: Internat. University Booksellers), 1957. Pp. 64. Price 10s.

It is no doubt natural that the achievement of independence by India should be marked by a general ebullience of interest in their own anthropology on the part of Indians. At any rate a considerable increase in ethnographical and sociological literature is taking place in India. There is still a vast field for investigation, and though change in the past has been very slow and gentle, it has probably been greatly accelerated in the past few years, and a great deal of traditional lore needs to be put on record now before it is lost for ever. It is quite fitting therefore that the output of tribal Research Institutes and of University Departments of Anthropology should be, as they are seen to be in the two bulletins under review, mainly what may be described as documentary. Thus the Chhindwara bulletin, among other items, contains a list of the kinship terms used by the Bhils of Malwa and a note on the Megnath Festival describing among other things a chastened form of the old hook-swinging ceremony (see Ives's *Voyage from England to India*, 1773, p. 27; Thurston's *Ethnographic Notes*, 1906, p. 487; Powell's 'Hookswinging in India' in *Folk-Lore*, Vol. XXV, p. 147), interesting as an attenuated if not actually vestigial survival. But the author of this interesting and careful record is not justified in describing tribal peoples as 'always living in supernatural atmosphere'; they don't; and the short article on the 'Care and Protection of a Baiga Child' is very naïve indeed. The Bihar University bulletin is very similar in general content. The editor, Sachchidananda, writes of the social structure of a Chotanagpur village; Professor Mrinal Roy describes the working of the *Parha*, which is the unit of territorial organization in the Munda tribe, and there are half-a-dozen other articles on Mundas, Birhors and Malpaharias.

Professor Chattopadhyay's *Study of Changes in Traditional Culture* is a more aspiring symposium, being the record of the proceedings of a series of conferences organized by the University of Calcutta at the instance and with the co-operation of U.N.E.S.C.O. to study the effects of modern industrial developments on the traditional cultures of Bengal. The conferences were convened and ably conducted by Professor Chattopadhyay, and the papers and discussions deal with the changes brought about or actually taking place, under the impact of modern communications and intellectual and economic developments, in tribal cultures, village societies and their organization, religious festivals, traditional

music, songs and rural handicrafts. The record is a depressing one, but the fact that the existence of practical problems resulting from such changes is appreciated and that solutions are being sought for the *malaise* which they occasion should do much to counterbalance the feeling of loss inseparable from a survey of the decay of familiar culture.

*Contributions to Indian Sociology* is also the first number of an irregular publication, but is published not in India but in Paris and has a far more ambitious aim. Its joint editors are sociologists, one of Paris, the other of Oxford, and they affect a new and particular approach to Indian sociological problems. The attitude which they adopt will probably commend itself to many Indians in that it lays emphasis, one might say excessive emphasis, on the unity of Indian culture. The remark of an earlier writer that India was merely 'a geographical expression' would be anathema to them. But their argument, that the traditional sanskrit civilization of Hinduism demonstrates the unity of India, might be used to extend the boundaries of that unity to include a great deal more of Asia and its islands than can possibly be labelled Indian, and it can be argued with at least equal reason that the diversity of language, custom and tradition within India herself demonstrates the undoubted fact that the culture of India of today derives from multiple sources. The editors make a number of shrewd observations such as that India has 'institutionalized inequality,' that the *panchayat* system of village administration is apt to be a failure because it attempts to apply to the village an institution developed only by the caste, or that Hinduism has 'repelled the onslaught of Christianity by integrating those Christian values which were most dangerous to it,' and they have no doubt a contribution of value to make to the study of Indian society. But if they are to base their approach on this presumption of unity they will need to beware of the pitfalls of inductive reasoning. It may be seriously questioned whether the assumption that Hinduism is derived from Vedism (an ambiguous term) can be taken for granted, that is if the religion of the *Rig-Veda* is meant, for there is some reason to regard Hinduism as older than the *Rig-Veda*, at any rate in India, while so learned an Indologist as Professor Tucci has suggested that instead of Jainism being a heresy from Hinduism it is the elder faith of the two. No one with knowledge of things Indian would suggest or ever has suggested that Hinduism is a 'mechanical juxtaposition of "Aryan" and "Dravidian" traits'; of course Hinduism is a 'historical creation,' but that implies that it has had growth and has a history; the spirit that informs it is no more static than Hindu society has been. It is perhaps their antipathy to the diachronic approach which makes the editors' long review of Mrs. Karve's *Kinship Organization in India* so highly critical of suggested shortcomings and so much less than just to her undoubted achievement.

J. H. HUTTON

**Mundari Folk Tales.** By P. K. Mitra. Ranchi (Education Press), [1956]. Pp. [xvi], 168. Price Rs. 5s.

**230** These folktales were collected by the author in the Mundari language, which he learnt when a civil servant at Ranchi some years ago, and are now published by him with an English translation. They appear to be of varied origin. No. 5 has many features in common with Perrault's *Le Petit Poucet*, and one of these is of particular interest. In both tales the seventh son, who with his brothers has been abandoned in the forest by their father, leads them by means of a trail of small stones. Saintyves, in his study of this tale, cites no Indian tale which contains this incident, and though in several European and African tales the hero achieves the same result with white ashes, it is only in France that he employs small stones.

There are other stories in which the seventh son is the hero, and some in which it is the third son. Rajas appear in a number of the stories, and in several the hero marries the raja's daughter and succeeds to his kingdom. The animal stories are unfamiliar except in that the animals converse freely, but we can hardly conclude from this as does the author, following Frazer, that early man believed that animals could talk.

The author is to be congratulated on his enterprise in collecting and publishing such an interesting and varied collection of tales.

RAGLAN









# SOME NEGROES OF ULCINJ, YUGOSLAVIA

(a, b) Djemo Shurla; (c) Mustafa Shurla (ob. 1941); (d, e) Rizo Shurla; (f) Dasa Shurla; (g, h) Nuho Shurla; (i) Ahmed Shepeteya.  
Photographs: A. Lopashich, 1956



# A NEGRO COMMUNITY IN YUGOSLAVIA\*

by

ALEXANDER LOPASHICH, DR. PHIL. (VIENNA)

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**231** The inhabitants of the small market town of Ulcinj on the southern coast of Montenegro in Yugoslavia have been known in the past as seamen, traders and pirates. They used to navigate and trade during the Turkish rule until 1877, and later on up to 1914 under the Montenegro flag in the Mediterranean. The Ulcinj seamen also traded in Negro slaves whom they bought, or induced to take up service in their ships by promising good wages, or inveigled aboard by various tricks; once in the ships they were not allowed to leave. The price paid for a Negro was 20 to 30 or even 50 gold coins. The poverty of parents is also mentioned as one of the reasons which made them sell their children. The traders usually bought these Negro slaves in the ports of Tripoli, Benghazi, Algiers, Philippeville, Tunis or Port Said. Near Tripoli there was a place called Gryani or Griyani (most probably Garian of today) whence many Negro slaves were sent to the Tripoli market. Slave-dealers in Tripoli, Benghazi and other places preferred to buy Negro children of seven to 10 years in the Sudan, paying for them with black cloths, sugar, tea or similar consumers' goods.

The Bagirmi country near Lake Chad is particularly mentioned as the region from which Negro children were bought for the slave market. I obtained these data (during my fieldwork among the Ulcinj Negroes in summer, 1956) from an Ulcinj man who used to be a trader in Benghazi many years before the Balkan war. The Ulcinj seamen usually left with their ships in the spring and returned to their home port in late summer or autumn. Often they brought back with them Negro boys and girls from two or three up to 16 years old. Some of these continued to serve as seamen in the ships of their masters, some were presented or sold to rich Ulcinj families, who employed them as servants in their houses or workers in their fields. The higher the number of slaves possessed by a family, the higher their reputation and their wealth were considered to be.

The names of sea captains who used to bring home Negro slaves are still known in Ulcinj. During the Turkish rule and the following Montenegrin period the sea captains were Hajibeg Dülü, Adem Hajimola, Haji Aliya, Haji Mehmed Betsi, Daut Suleyman Bužina, Lito Brashnye, Smail Ukoshata (Turkish rule) and Selim Shurla, Tahir Shurla, Hasan Shepeteya, Husein Bechir Aga and Yusuf Kaplanbeg (Montenegro period).

Captain Suleyman Bužina brought three Negroes from Tripoli: Aliya, Ahmed and Omer Sala. The first two were 15 and the last 10 years old. Captain Smail Ukoshata brought a 16-year-old Negro slave, Ibrahim Mehmed, who died 27 years ago, but his son, now over 70 years old, is still living at Ulcinj. Captain Lito Brashnye brought the

Negro slave Abdula, whose son Rizo, now about 50 years of age, used to breed cattle. Captain Fitsi brought a Negro couple, Mergian and his wife Hazize. Mergian succeeded in living, it is stated, to be 120 years old. The daughter of Mergian, Bibe, was married by the above-mentioned Ibrahim Mehmed Ukoshata. Captain Fitsi brought also to Ulcinj the Negro Musa 'Arap,' whose grandson is still living at Bar (Antivari), a port some 20 miles north-west of Ulcinj.



FIG. 1. RIZO BRASHNYE

In 1877 Ulcinj was occupied by Montenegro. This induced Haji Beg Dülü and Haji Mehmed Betsi to emigrate to Albania taking with them eight and five Negro slaves respectively. During the Montenegrin rule Husein Bechir Aga brought from Tripoli the Negro slave Ahmed 'Arap.' Having no children, he intended to adopt him, but Ahmed escaped from his ship in 1910 or 1911 and returned to Africa.

About 55 years ago Captain Tahir Shurla brought from Tripoli and Griyan (Garian) respectively his Negro slave Said of about 14 years of age and in 1905 another one, Mohammed by name. Said died in 1941 and Mohammed in 1948 at Shtoj near Ulcinj. Said's children are still living at Ulcinj.

Fifty years ago Captain Hasan Shepeteya brought home the old slave Yusuf who died about 1926. Yusuf married an Ulcinj Negro woman, by name Bude, who died in 1933; her son Ahmed lives at Ulcinj and is a mechanic.

During the Turkish rule there were about 100 Negro families at Ulcinj. In 1877, however, the invading Montenegrins found only about 50 families with nearly 150 members. Later on, according to local oral traditions, there were about 30 Negro families. They lived at Ulcinj, at nearby Shtoj, also at Bar (Antivari) and at Scutari in Albania. In addition to the Negroes already mentioned, whose sons or

\* With Plate N and three text figures



descendants are still living at Ulcinj and Bar, other Negroes are still known by name although they had died during the Montenegrin rule. Among them were Kaso 'Arap,' Abdul 'Arap' and his wife Fata, both of whom died during the First World War; then Avdal 'Arap,' brought by Captain Mula Daut Hoja, and Yusuf Beg's seaman Ahmed, who both came from Tripoli; Husein Beg's slave girl Zahra, brought from Alexandria, and Amza 'Arap' of Bar are also still remembered.

At the end of the nineteenth century Musto 'Arap' died in Ulcinj at the alleged age of 124 years. He had served as seaman in a pirate ship, the *Sokoleva*, and in one of their adventurous undertakings was the only one who survived. Since he had been declared dead, it took him a considerable time after his return to convince his mother and neighbours that he was the real Musto 'Arap.'

The best-known among them was Yusuf Beg's Maksut 'Arap.' He is still remembered by many people who were told by him that he had been born in Africa as a son of a Negro chieftain (according to some informants Maksut came from Algeria). When only 12 years old he was taken prisoner by a neighbouring Negro tribe at war with his own. Maksut was then brought to Tripoli and sold to Bechir Yusuf of Ulcinj. He was at first a slave at Ulcinj, but was later freed by his master Bechir Yusuf. He married and even got a horse from his former master, and started to earn his living by transporting goods between Ulcinj and Bar. At the jubilee festivities at Cetinje in 1910 of the ruling Knez Nikola II and his proclamation as King of Montenegro, the stout imposing figure of the Negro Maksut 'Arap' was prominent among the guests. Knez Nikola offered him the post of one of his *Perjanik* (members of the king's bodyguard). Maksut, however, declined this honour, excusing himself by the fact that he could not speak the country's language well and lacked the training for such a post. Besides, he would have temporarily to give up his job, for *Perjanik* were replaced every month. After the First World War his master took him to Istanbul, since his wife and daughter had died and he was left alone without any relatives. The older people of Ulcinj still remember him very well and all the inhabitants came out to see him off when he was leaving for Istanbul. Maksut was a very respected person and he always played the role of arbiter in cases of quarrels among the Ulcinj Negroes. In Istanbul his master Bechir Yusuf did not take much care of him. Being old and unfit for work he returned to Ulcinj, where he died in October, 1923.

All these Ulcinj Negroes came as children to that place and they therefore did not remember the names of their families, and took the surnames of their masters, for instance Ibrahim Mehmed Ukoshata or Yusuf Shepeteya. The word 'Arap' denoted in this case a slave rather than a Negro, and referred to the colour of their skin, although they had no relation whatever with Arabs. (The Turks also used the word 'Arap' for Negro.) The freed slaves among the Negroes used to have the word 'Arapi' as their surname, as, for instance, Yusuf 'Arapi' or Tahir 'Arapi.' Yusuf 'Arapi' was the father of Ahmed Shepeteya. These Negroes usually served as slaves for a certain number of

years and were then freed by their masters. They were commonly given a small house and a piece of land by their former masters, but when freed continued to remain under their protection. The children of these freed slaves were considered free citizens. The masters would often present their former slaves with legacies in their last wills. All these Negroes served as seamen in the ships of their masters, in their fields, or as servants in their houses. Rich Ulcinj tradesmen even used to give some slaves as a part of their dowry to their daughters when they married. For example, it is known that the grandmother of Adem of Bar (Baranin) had four slaves when she married.

The freed slaves worked either in their fields or in ships as seamen. I was told that the Negroes were not allowed to engage in crafts; however, two of them, Guchi Ruhes and Daut Kaliya, were well known as captains and owners of small ships. Guchi Ruhes was master of a small ship but before the First World War he emigrated to Scutari where he died. His father was brought from Tripoli. The latter, Daut Kaliya, was the owner of one of the largest Ulcinj ships. In the eighties of last century he was burned to death in his ship in the Bojana river, the outlet of Lake Scutari to the Adriatic. Daut Kaliya possessed a very beautiful house at Ulcinj which is inhabited even today. I have visited it and found in one of its rooms a well preserved ceiling decorated with wood-carvings of considerable artistic value. That ceiling was the subject of praise in songs sung by Negroes and later even by the Ulcinj people too.

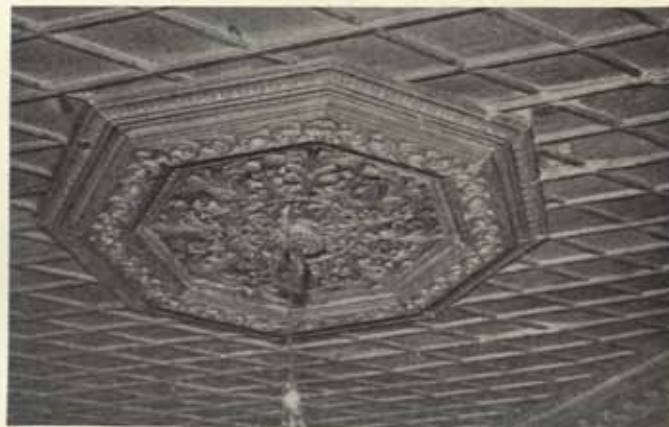


FIG. 2. CEILING WITH WOODCARVING IN DAUT KALIYA'S HOUSE

Thus I was able to take down two songs in the Albanian language adopted also by the Negroes when brought to Ulcinj:

*Sipokerset shamatatezi*  
*Bain Harapat leli-leli*

While the ceiling is shaking  
'Arabs' are dancing leli-leli

or:

*Daut Kaliya, Smail Mezürü*  
*Bain odat si Nuro Dülü*

Daut Kaliya [and] Smail Mezürü  
Build rooms like Nuro Dülü

Nuro Dülü was well known for its wealth.

Negroes brought to Ulcinj spoke Arabic, of which they remembered and retained some words, e.g. *ash hālek*, how



do you do?; *tabim* (ta'zum?), a greeting; *lahm*, meat; *hubz*, the bread; *kuli*, all; *mafish*, there is not. However, it is not known whether any of the Negroes brought to Ulcinj spoke a Sudanic language there. Perhaps a Sudanic vocabulary was preserved in the Arabic spoken by Negro children brought to the Mediterranean coast of Africa, but none of the Negroes and seamen living today at Ulcinj were able to answer this question. Those brought as children to the coast might have preserved some words, but in the course of time they presumably forgot them, the more so because at Ulcinj the Albanian language became the mother tongue of their children. There is, however, a song which they used to sing at their meetings containing what would seem to be Sudanic words:

*E ya ya yo Bagirmi nago*  
*E ya ya yo danga nigano*  
*E ya ya yo sema barka<sup>1</sup>*

In this song the Bagirmi country is mentioned, from which the Negroes were most probably brought to the Mediterranean ports. More cannot be said about the song until the origin and the meaning of its words have been discovered.

The Ulcinj Negroes married only Negro girls, either of Ulcinj or brought by ships; they did not marry Albanian girls, except in very rare cases. According to the still recognized tradition marrying a non-Negro girl was considered an offence and a *mésalliance*. This is how they have succeeded in preserving their racial characteristics until the present time. Some years ago, two Negroes married white girls, but these marriages remained without offspring. There is also a case of a Negro girl who married an Albanian, and now has two children of one and three years of age. Their skin is of a lighter shade; this will, no doubt, accelerate the process of their disappearance and assimilation with the Albanians.

These Negroes used to meet to celebrate various festivals outside Ulcinj, on the flat top of the hill called Pinyesh, and this is still called Fush-Arabi, i.e. the field of the Arabs. Both the freedmen and the slaves used to meet there. Bairam was one of the festivities particularly mentioned. They sang and danced accompanied by drums and by the *zurla*, an oriental kind of flute. During last century, up to 300 Negroes usually gathered there, not only from Ulcinj but also from the surrounding areas. The freedmen brought to the feast what they needed, whereas the slaves were presented with appropriate food and drink by their masters. The Negroes enjoyed the traditional kinds of fancy bread, *halva* and *sarailiya* (a sweet pie made of dried grapes, sherbet and flour); they drank sherbet, lemonade and coffee. These dishes and drinks were the usual Fush-Arabi menu, but they also liked to eat the pungent paprika called *fil-fil*, meat and *mulaki*, spiced small pieces of grilled meat. Fush-Arabi was also the scene of their games with sticks and lamb bones. The native cap was put on the top of a stick and had to be thrown off from it by means of a blow with another stick. In their houses the Ulcinj Negroes also played a game consisting of hitting the ceiling of the room with a stick according to certain rules. The game with lamb bones (called *ashik*) consisted of trying to

hit one lamb bone with another. Those who succeeded in hitting the bone won the game and the losers had to pay the cost of the prize consisting of a cup of coffee, sherbet or lemonade. Spirits were not drunk, judging by all information received. These meetings in the Fush-Arabi, which are still remembered, always ended without any disturbance. The Ulcinj Negroes would also meet at the café Trumo, also called the 'Arab café' because the keeper and customers were exclusively Negroes; another café, that of the Negro Yacub, also had Negro customers. The Negroes were known for their bravery and boldness, their physical strength, good health and endurance. They liked to dress like town people, showing rich adornments. Men and women followed the fashion within the limits of their means. Though known for their kindheartedness, they were also much feared when in a bad temper. For example, Maksut Bechir Yusuf and Mohammed Shurla were feared for their strength and their occasional rages. Even the present generation is still physically very fit; for instance, Nuho Shurla managed to thrash a group of gendarmes in an inn some 20 years ago. In spite of the new environment and a different social atmosphere, the Ulcinj Negroes succeeded in retaining some of their characteristics, such as lightheartedness, fondness of music, rhythm and fun, love of family life and a certain amount of personal attractiveness.

It is interesting to note that they failed to have numerous descendants either on arrival at Ulcinj or later on. The mortality rate among them was also considerable.

The Ulcinj Negroes have been and still are Mohammedans by faith. Their women have always hidden and still hide their faces and decline to give up that custom. According to information which I received from the Albanians of Ulcinj, some customs of the Ulcinj Negroes are apparently either of Arab or African origin, since they are not Albanian. For example, trousers were not allowed to be left near the bed because otherwise the owner would dream while sleeping. This custom was told me by Rizo Brashnye who himself had it from his father and still observes it. He was further told by his father that if he wanted to do harm to a person he must appear in the dreams of that person. If a person appears in the dream of another, the dreamer has only to turn the pillow over and then he himself will appear in the dream of the former one and then will remain undisturbed. It is still remembered that Negroes brought to Ulcinj had scars on their faces indicating their tribes. Two scars indicated the town dweller, while one scar was the sign of people living in the country. It is known that Abdula Brashnye had two scars and Mohammed Shurla only one scar across the face. Negroes without scars on their faces were considered to be of lower rank.

*Genealogy I.* A particularly interesting person was Zahra Haji Halil Fitsi, who had been brought from the Sudan. Her husband Haji Halil Fitsi was a Bimbashi (major) in the Turkish army and served in the Egyptian Sudan during the Turkish sovereignty. There he married the Negro girl Zahra, also called Yume (meaning 'mother' in Arabic). About 1877 he returned to Ulcinj with her and their



daughter Zülükaya. This was at the time when Montenegro occupied Ulcinj, and Dervish Pasha in Scutari invited all Turks living in the coastal region then acquired by Montenegro to emigrate to Albania or some other part of the Turkish Empire. After settling down at Ulcinj Haji Halil Fitsi, according to tradition, married an Albanian girl; before leaving the Sudan he had told Zahra that he would do this to comply with his parents' wish on his return to his native Ulcinj. He suggested that she should go with him but agreed that she might remain in Sudan if she did not want to leave it; but in any case he wanted to take with him his daughter Zülükaya. At Ulcinj Zahra left Haji Halil Fitsi and married the Negro Yacub who used to keep the small café, frequented by Ulcinj Negroes, which I have already mentioned, and which he was given by Haji Halil Fitsi. Yacub died about 60 years ago. Zahra wore golden rings in the septum of her nose and was particularly known, as is well remembered, for her ability to tell the future from the muscles of the right arm. If a person was robbed of a thing he came to Zahra and told her the names of suspected thieves; while listening to these names she used to repeat the word 'angala' and when the name of the thief was pronounced then the muscle of her right arm would bounce up several times. She learned that ability in the Sudan from *Mulegin* (sanctifiers). Zahra was also known for her rich and precious jewels brought from the Sudan. From time to time she was seized by fits of nostalgia for her native country, and on such occasions would start dancing the belly dance and singing the following song:

*Jene jevap minel  
Khartum minel mou<sup>2</sup>  
Zaga zaga ru ha fadalaim  
Fatna ebu ma maksusa  
Fatna be seré seré  
Kumusha fadalaim*

She also sang a song from the Sudan imitating the buzzing of mosquitoes:

*Nyenjes bulango  
Ilentin tumu Kuran  
Tsakalu wa nye.*

The words *bara* and *kalu* are still well remembered and apparently the former means: 'go, get away' (Arabic). Further a lullaby was sung by the Negro servants to Haji Halil Fitsi:

<i>Natlap min Allah</i>	I ask from God
<i>vali mush ketir we</i>	And for myself not much
<i>barka buluri ismu</i>	Blessing by the light? of the name
<i>Halil</i>	Halil. <sup>4</sup>

Zahra died in 1917 or 1918, in her one-hundred-and-sixth year. Her daughter Zülükaya had five daughters: Nuriya, Ruhiya, Esma, Fariya and Beyaze. Esma became the wife of Selim 'Arap' of Bar whose father Musa 'Arap' was brought from Africa. Their son Jemal, already mentioned, was born at the village of Zavalje near Bar; he already displays such changes in his appearance that he hardly differs from the Albanians of the region. Beyaze is still alive and remembers her grandmother Zahra as well as her songs; a slightly darker skin and some small details in the face are the only signs of her Negro ancestry.

Selim Beg of Bar, who died during the Turkish era, had a Negro girl in his harem who later on died in Albania.

*Genealogy II.* Ibrahim the father of Said Ukoshata told his son that he had been brought to Ulcinj from Tripoli in his sixteenth year. Ibrahim served in the Turkish army in Tripoli. One day he was walking about in the port of Tripoli and was approached by Captain Smail Ukoshata of Ulcinj, who lured him to join his crew as a sailor by promising him a good wage. During the enquiries and checks made by the Turkish authorities, he was kept hidden among the goods in the hold. In this way Ibrahim came to

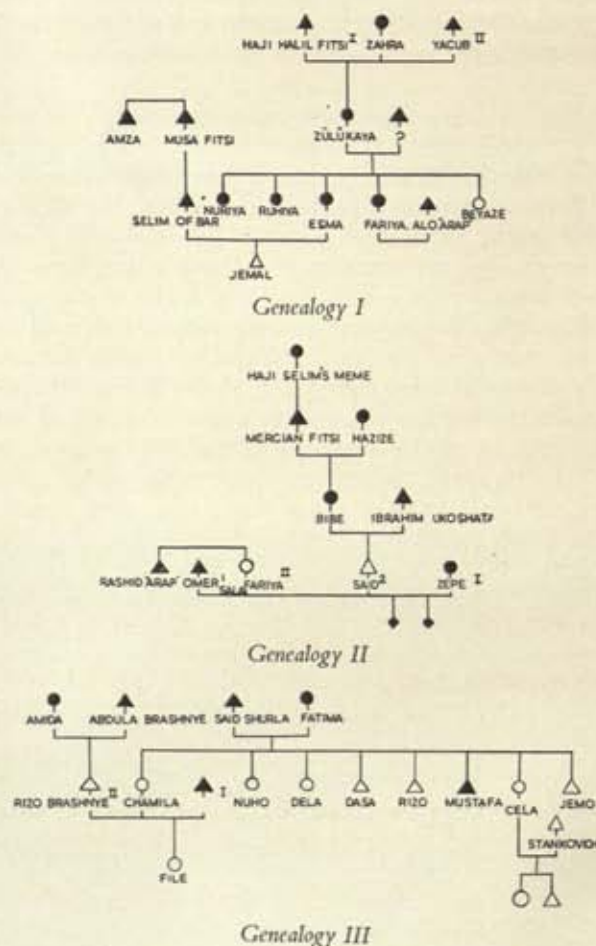


FIG. 3. GENEALOGIES OF ULCINJ NEGROES

Ulcinj. At first he served as a sailor in the ship of his master, but later on used also to tend his cattle. He was also given a house by his master. Ibrahim married Bibi, the daughter of Mergian and Hazize, who were brought, as already mentioned, to Ulcinj from Africa. Mergian's mother was called Haji Selim's Meme. Ibrahim died about 27 years ago in his eighty-fourth year. His son Said also worked as a sailor in Smail Ukoshata's ship and with the Fitsi family, and is therefore also called Fitsi. Said Ukoshata still remembers very well the arrival of Mohammed Shurla who came to Ulcinj about 1905. He married twice. His first wife Zepe died in Albania as did their two children. His second wife Fariya, now 67 years of age, was first married to Omar Sala, who has been already mentioned.



Her brother Rashid 'Arap' had already died during the Turkish rule.

*Genealogy III.* Abdula Brashnye was brought to Ulcinj while it was still under Turkish sovereignty. He married Amida, an Albanian girl, and their son Rizo is now about 50 years old. Abdula Brashnye was a farmer at Shtoj near Ulcinj where he also bred cattle and grew corn and vegetables. At first he, like his son Rizo, used to work in Smail's ship. Rizo, however, is today a farmer at Shtoj. His skin is already lighter, his hair is very thin and in other ways also he shows signs of his mixed ancestry. Rizo married Chamila, the sister of the Shurla brothers, who is now 56 years old. She also has a daughter File by her first marriage, who is now 24 years old. Abdula Brashnye knew the place from which he was brought to Ulcinj, but his son Rizo forgot its name. He only remembers that his father was brought to Ulcinj by means of a trick.

As has already been mentioned, Captain Tahir Shurla brought the Negroes Said 'Arap' in the nineties and Mohammed 'Arap' about 1905 (see *Genealogy III*). Said Shurla was a farmer growing corn and breeding cattle at Shtoj, a hamlet near Ulcinj. He married Fatima of Ulcinj, and had eight children. His first son was Nuho, now 38 years old, a fisherman and farmer by profession. His second son is Rizo, 35 years old, a former boxer in Zagreb, but now a photographer at Ulcinj. The third is Jemo, 28 years of age, working as a waiter at Dubrovnik and Ulcinj. The fourth son, Dasa, completed his studies at the Dubrovnik School of Navigation and is now living in South America. The fifth son, Mustafa, died at the age of 16 in 1941 in Zagreb. Said's daughters are Chamila, now 56 years old and married to the already mentioned Rizo Brashnye; by her first marriage she has the daughter File. The second daughter, Dela, 35 years old, is still a spinster, while the third daughter, Tsela, 28 years old, is married to Stankovich, a Mohammedan of Ulcinj; she has two children of three and one year respectively.

Said Shurla died in 1941 in his sixty-fifth year, while his wife Fatima had died in 1935 in Dubrovnik. It is said that he was brought as a 14-year-old boy from Tripoli, but it is also said that he was brought as a child of three years to

Ulcinj. The Shurla family is the most numerous Negro family of Ulcinj. They have retained their racial characteristics, a very dark and almost black skin, the typical curly hair, thick lips and physical strength. Ahmed Shepetaya, too, still shows these racial characteristics although he is dark brown rather than black, while Said Ukoshata, the tallest of them all, possesses all the Negro characteristics, but his skin is somewhat lighter and he has an elongated skull. Among the Ulcinj Negroes Rizo Brashnye is the one who apparently remembers most stories told him by his father and has retained most of their customs. Ahmed Shepetaya's brothers Nazmiya and Rejep died in Albania.

Judging from what has been stated so far, the Ulcinj male Negroes succeeded in accustoming themselves to the way of life of their new environment and the native population accepted them gradually as their own people. The female members of their families, however, remained separated and have even continued to hide their faces till today, although that custom has been abandoned by the Mohammedan women in Ulcinj. They strictly observe Mohammedan customs (and because of this I have not been able to photograph them). It should be added that near the salt works, there is the grave of an Ulcinj Negro called Mohammed 'Arap.' At Ostros in Krayina (the region between Ulcinj and Lake Scutari) in an ancient Mohammedan burial place is the grave of another Negro; it is 3 metres long and is called *Vori Harapit* (Arab grave) by Albanians of the district. According to tradition an 'Arap,' i.e. a Negro, was buried in it.

#### Notes

<sup>1</sup> Mr. D. Cowan, School of Oriental and African Studies, translated 'sema barka' as 'heaven's blessing' (Arabic).

<sup>2</sup> Mr. D. Cowan, School of Oriental and African Studies, London, translated the first and second lines:—'There came a letter from Khartoum from . . .'

<sup>3</sup> Professor A. N. Tucker, School of Oriental and African Studies, tells me that *nyenje* means a mosquito in Moru-Madi (a group of Eastern Sudanic languages in the Southern Sudan).

<sup>4</sup> Translated from the Arabic by Mr. D. Cowan.

## THE BOSKOP 'RACE' PROBLEM\*

by

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**232** Because of the increasing quantity of material being discovered in Africa, and because of the great surge of interest in these anthropological discoveries which tend to emphasize evolutionary trends, it is considered reasonable to present certain facts and discrepancies concerning the use, misuse and abuse of the Boskop concept,

\* With two tables. A paper read in modified form at the V International Congress of Anthropological and Ethnological Sciences, Philadelphia, Pa., in September, 1956

which has figured in so many publications concerning fossil man from Algiers to Cape Town during the last 40 years.

A complete survey of the Boskop problem would require at least a monograph. Consequently I propose only to indicate how, on the basis of an isolated cranial fragment found 40 years ago near the surface in a dubious geological horizon, unassociated with implements and fauna, there has been developed conjecture after conjecture, speculation on speculation, until today one finds physical



anthropologists who have not only constructed a 'race' around this skullcap, but who also detect occasional features of this 'race' in the faces of living South African *individuals*. The confusion is intensified still further by claims for pre-Bush, proto-Bushman, proto-Australoid, pre-Hottentot, and other 'pre-' affinities in skeletal material when adequate definition of and agreement on what a Hottentot or Bushman physical type is does not exist. What does it mean when we read: 'When the Kakamas folk arrived in Southern Africa, they found a numerous pygmæopedomorphic people, the Bushmen, compounded mainly of the Bush and Boskop physical types, with a slight admixture of a gerontomorphic strain and possibly an earlier Europoid strain; they also found some relatively unhybridized Bush, Boskop and Gerontomorphic types' (Tobias, 1955)? What do 'relatively unhybridized' types look like? What is a 'slight' admixture of a gerontomorphic strain? This statement was made on the basis of about 50 skulls of the Kakamas collection which provide a spectrum of features which may be called Hottentot, Negroid and Bushman. There was no evidence of stratification or culture (Dreyer and Meiring, 1937), and yet conjectures are made of the 'arrival' in Southern Africa of this 'racial type' when there is historic evidence that many of these Kakamas people are recent admixtures of Hottentot, Bushman and Bantu, although Tobias considers the Kakamas types 'significantly different from both the Bush and the Boskop races.' This illustration indicates the confusion and the way in which theories are easily evolved without facts. Again, in reading through accounts of discoveries in Southern Africa, one comes across the Wilton 'race' and the Matjes River 'race' (both regarded by some as being part of the Boskop 'race'), the Bush 'race,' the Fish Hoek 'race,' the Oakhurst People (or race), pre-Bushman Races, the proto-Australoid race, etc. Broom labelled the Boskop skull *Homo capensis*, Dreyer called the Matjes River 'race' *Homo sapiens dreyerensis*, and so on. These 'races,' species and subspecies are based on what are, relatively speaking, infinitesimally small collections of skeletal material, unearthed in scanty groups or singly over thousands of miles of territory.

It is still a failing among not a few anthropologists that they feel it incumbent upon themselves sooner or later to plant an evolutionary tree, to construct ancestral branches, to designate apparently unusual features in a skull as 'primitive' or pre-this or pre-that, and to plan vast migratory routes of so-called prehistoric 'races' which are represented only by odd skulls.

These are the reasons why I am at present carrying out a survey of all Southern African skeletal material under the sponsorship of the Wenner-Gren Foundation, so as to try to determine their fundamental features and attempt a classification based upon acceptable and factual definitions. I will now briefly elaborate what I consider to be the basic general discrepancies in the concept of the Boskop 'race' or what has been called the 'Middle Stone Age Physical Type.'

In 1913 a part of a calvarium was unearthed by labourers on a farm near Boskop, a village in the Potchefstroom

district of the Transvaal (FitzSimons, 1915). The specimen consisted of a greater part of the frontal, parietals and a small portion of the occipital. The exact spot of the specimen was not determined by the investigators who visited the farm shortly after, but it appeared to have been found in the surface lateritic deposit at a depth of about four feet below the surface, and there was no evidence whether the remains had been in a prepared grave or not. A year later excavations at the spot resulted in the discovery of a nearly complete temporal bone, most of the horizontal ramus of the left side of a mandible and a number of fragments of limb bones. The site was in a cultivated field about 250 feet from the bank of the Mooi River and about 12 feet above the river-bed level. No faunal or cultural remains were found in direct association, except for a single unusual artifact (van Riet Lowe, 1954).

Right from the moment of discovery these fragments were surrounded by an aura of antiquity: the first report by FitzSimons (1915) compared them with Neanderthal man, and though admitting differences he concluded 'that the Boskop Man was of the Neanderthal race, but more advanced in intelligence than the type specimen.' In 1917 Haughton commented that the temporal bone displays 'undoubtedly primitive features pointing to a pithecoïd morphology comparable with that of *Homo neanderthalensis* as exhibited in the skull of La Chapelle.' He considered the skullcap 'comparable to the Negroid and Bantu types though it seemed closest in size and general characters to the Cro-Magnon types.' He also considered the mandible to be in a slightly more advanced stage of evolution, and to be comparable on the whole to the Bantu. Haughton's criteria of the primitive nature of the temporal bone rested mainly on the length and strength of the supra-mastoid ridge. He also made a number of comparisons, which need not be severally commented on here, but in which features were qualitatively compared with Neanderthal and Cro-Magnon skulls. It need only be added that most of the features mentioned, including the supra-mastoid ridge, may be observed within any group of modern skulls, certainly in the Cape Coloured, Bantu, Eskimo, American Indians and European crania examined by me. One of the striking features mentioned in Haughton's paper and in many subsequent papers dealing with so-called Boskopoid types is the median depression between the parietal bosses. Now, in many skulls observed by me there is a slight exocranial heaping-up on each side of the sagittal suture during active growth and this normally leaves a relatively negligible groove in the region of the suture itself, and this groove broadens out towards lambda. (This heaping-up effect is more marked endocranially.) With total closure of the suture this region may present a slightly scooped-out effect: identical 'median depressions' have been observed in all the above modern groups examined. Furthermore, it must constantly be kept in mind that a fair degree of distortion is present on the left side of the Boskop calvarium. This is important when one considers to what degree parietal bossing is present. It certainly is suggestive on the left, while on the right it is less so, and it appears as if bossing was in a minor degree.



Haughton and subsequent Boskop-champions placed great emphasis on the length and prominence of the central 'crista frontalis' on the internal aspect of the frontal bone. This feature has been seen in all modern racial groups occurring as an individual feature of varying degree from very marked to complete absence.

Another Boskop feature always emphasized is the excessive thickness of the skull. Haughton's figures are 7 millimetres at the posterior inferior angle, 13-14 millimetres at the parietal boss and 10.5 millimetres at obelion. Because of the obliteration of the sutures and absence of parietal foramina, obelion is difficult to locate, but in any event on the endo-cranial aspect of the posterior half of the sagittal suture region the bone is heaped up as is commonly found in any series and this has given an exaggerated thickness. Para-sagittally the reading is 8-10 millimetres, although at only one point 6 centimetres to the right of the midline near the parietal boss it is 13 millimetres. Just as the thickness of the frontal bone is exaggerated opposite the glabella and at the positions of the primary ossification centres, and at the external occipital protuberance, so it would be where parietal bossing is present. Random readings taken elsewhere on the calvarium give a range of 7-11 millimetres, which is well within range of modern groups of crania (Table I).

TABLE I. SKULL THICKNESS: RANGES

	European (German) (42)	Cape Coloured (100)	Bantu (148)
Glabella	11.2-21.5	6.0-24.0	6.1-22.0
Mid-Frontal	{ L. 3.0-13.0 R. 3.2-13.4	1.8-10.0 1.7-11.1	3.0-14.0 3.0-14.0
Opisthion	3.0-8.2	1.9-12.9	3.0-13.9
Occipital	{ L. 1.3-7.2 0.6-4.0 R. 1.3-7.3 0.4-4.0	1.1-7.9 0.4-6.4 1.0-7.4 0.4-6.3	2.0-8.6 0.2-7.7 2.0-9.2 0.8-7.0
Euryon	{ L. 2.0-9.4 R. 1.2-8.2	2.2-10.0 2.2-10.0	2.1-10.0 3.1-9.2
Vertex	3.3-13.0	3.1-9.2	3.0-10.0
Mid-Parietal	{ L. 4.2-11.1 R. 4.0-11.2	3.0-10.2 3.5-10.1	3.9-12.3 3.0-11.3

Note that the occipital readings are taken at the midpoint of the cerebral fossa (upper row) and the cerebellar fossa (lower row)

In 1925 Pycraft of the British Museum reviewed the Boskop fragments. He asserted that the eminentia articularis and the glenoid cavity are Bushmanlike, but if one examines a series of Bushman, Hottentot and Bush-Hottentot crania, it is clear that these Boskop features are common to individuals of all three groups. Along sound lines he estimated the cranial capacity as only 1,700 cubic centimetres rather than the 1,832 of Haughton and the 1,900 of Elliot Smith and Broom. In addition to indicating numerous similarities to Bushman and 'Strandloper'

(which are in fact Bush-Hottentot remains), he too turned to Cro-Magnon for comparison, although the position of the mandibular condyle suggested on the temporal bone by the glenoid fossa and the reconstruction of the approximate position of the mandible indicate very strongly an orthognathous type of face. Whether this was big, as I think possible, or small, as Tobias, Wells, Dart and others aver, is difficult to say. For instance, in Hottentot and Bush-Hottentot skulls with an almost identical calvarium to that of Boskop I have observed either pedomorphic or gerontomorphic features.

The small teat-shaped mastoid process is of the type commonly found in many modern Bushman or Bush-Hottentot crania.

The frontal bone is rounded and narrow with a constriction or type of ophryonic groove above supra-orbital ridges which have a marked lateral angle—all typical features of many Bush-Hottentot and Hottentot crania. It is not possible in this preliminary report to discuss any of the other numerous so-called Boskopoid features, many of which never occurred in the Boskop calvarium *per se*, but in crania which in *norma verticalis* resembled the Boskop type. Such are a wide, shallow palate (Galloway, 1937a), various post-cranial skeletal characters (Gear, 1926), supra-asterionic bulging of the parietal bone (Wells, 1934), and the deep groove for sigmoid sinus (Wells, 1934), etc. However, each of these can be shown to be not a specific feature of either this one skull or any one 'new' racial group. What then are the distinctive features of the Boskop calvarium? All that are left are its relatively great length and breadth.

The great length and width have become the *sine qua non* of Boskopoid types according to Gear (1926), Galloway (1937a, b), Dart (1940) and Tobias (1955). Galloway (1937a) defined a Boskop type of skull as one exceeding 190 millimetres in length (often 200 millimetres) and with correspondingly great capacity (usually 1,500 cubic centimetres). Dart initiated the concept of a Boskop physical type in 1923 after a preliminary study of the poorly fossilized Zitzikama remains, which are undoubted Bush-Hottentot remains found with their recognized cultural associations. These remains proved, he stated, that the fossilized Boskop type was representative of the type which had been widely dispersed in South Africa before its occupation by the Bushmen. Subsequently Dart's students took up this theme and expanded it into the 'Boskop race.' The main exponents were Gear (1926) and Galloway (1937a, b). Dart (1940) gave the final statement of the case.

It is not clear how the 'Middle Stone Age Physical Type' (which became synonymous with the Boskop 'race') came into existence in the literature. Probably it was the result of the view that Boskop was pre-Bush, and subsequently, when the South African Middle Stone Age was defined, it came to be identified with the Boskop 'race.' However, when Wells (1947) made a thorough study of all those remains found in association with M.S.A. cultures, it became obvious from his report that a common 'type' did not exist. Wells, who was originally an exponent of the Boskop 'type,' began to express a change of his



views (in print) in his First Pan-African Congress on Pre-history paper (1947) and in a summary of a communication to the Royal Anthropological Institute (MAN, 1952, 52). It is now clear that what Orford and Wells (1936) described as 'Boskop' features (which they considered to have been absorbed by the 'Bush' group and retained by the Bantu population) are actually Hottentot features, but they were then accepting Dart's teaching, which did not recognize 'Hottentot' as being physically distinct from 'Bushman,' both being lumped together as 'Bush.' Galloway (1937a) was the first to determine the Boskopoid 'range' on a number of skulls and post-cranial remains (Zitzikama, Fish Hoek, 3 Matjes River skulls, Springbok Flats and U.C.T. 80), and thereafter the Boskop 'type' characteristics were assessed thereby without re-investigation of the conclusions.<sup>1</sup> In that publication Galloway stated: 'During the past few years, workers in this department have slowly been piecing together an imaginary picture of the skull of the Boskop physical type.' Jones (1940) used the term 'Boskop type' to embrace all pre-Negro types which are more robust than the Bushman. Wells (1950) now wishes to confine the term only to one out of several robust pre-Negro types. He adds, nevertheless, that these types all appear to be fundamentally related to the Bushman with whom they may be bracketed as constituting a 'Bushmanoid' variety of modern man. The height/length ratio is equivalent to that in the Bushman, the height being in the range of modern forms. The breadth/length ratio, breadth being 75 per cent. of the length, is common to Bushman, Hottentot and Bush-Hottentot. The prominence of the frontals is a Bushman feature, although the supra-orbital constriction appears also in Hottentot or Bush-Hottentot crania. This dolichocephaly and relatively low vault is a feature found in the Kakamas Hottentots, some Bush-Hottentot hybrids, Negroes and Negroids. Why the large-headedness should be referred to as *pre-Negro* when no fossil Negroes have been found in Southern Africa is difficult to understand.

Consequently, in terms of African racial types, the features exhibited by the Boskop skull and those which have been termed 'Boskopoid' are not specific to any 'new' single, African racial group, and in Africa they may

be found in varying degrees in the Bushmen, Hottentots or Bush-Hottentot admixtures, the emphasis being on the Hottentot strain. The question of the degree of involvement of a Negroid element in the make-up of the Boskop skull is debatable. It is significant that wherever so-called Boskopoid types are considered to have been found, whether in mountain caves or coastal plains or rock shelters, the dominant features of most of the other crania in the same site were either Bushman or Hottentot; moreover, when a cranium had one or more of these features exaggerated they were called Boskopoid, and indeed the large skulls seem to have been selected from these excavations and labelled 'Boskopoid.'

It is unrealistic to create a new racial type in the presence of other types which adequately explain its genetic make-up. The known modern Bushman and Hottentot types must be used to appraise the fossils, because they too occur as fossils, and where large numbers are found in strata, as for example at Matjes River, the various layers produce varying degrees of Bushman and Hottentot features (and possibly Negroid features) in the skeletal material.

If one examines those crania which have been labelled Boskopoid one discovers the illogicality of the Boskop 'race' concept. In order to establish a Boskop 'race,' identical features in a large series of skulls must be attributed to the Boskop skull itself, but the only specific feature of the Boskop skull is its great length and width, the width falling well beyond the normal outer limits of the Bushman and of the Negro, the length being within the ranges of the Hottentot and Negro. The following list includes a few well-known sites, skulls from which have been attributed to the Boskop 'race' by various authors and by the Committee (Dart, Drennan, Tobias and Robinson) who drew up the Catalogue of Fossil Man for the XIX International Geological Congress held in Algiers in 1952 (published in 1953): Plettenberg Bay, Matjes River, Springbok Flats, Fish Hoek, Cape Flats, Kalk Bay, Oakhurst, Zitzikama and University of Cape Town dissecting room (skull No. 80). If it is size of the calvarium only that should determine the Boskop 'race,' then most of these compatriots of the Boskop skull have little in common (Table II), and their related facial and post-cranial metrical

TABLE II. COMPARISON OF 'BOSKOPOID' AND OTHER SOUTHERN AFRICAN SKULLS

	Boskop	U.C.T. 80	Cape Flats	Fish Hoek	Matjes River (9)	Zitzikama (5)	Outeniqua (12)	Zuurberg	Oakhurst (9)	Total Range Boskopoids	Hottentot (35)	Bushman (31)	Bush- Hottentot (99)	Bantu (220)
Maximum Length ..	205	212	191	198	184-195 (191)	181-210 (197)	166-192 (180)	178-185 (181)	174-190 (181)	166-212 (191)	180-202 (192)	165-181 (175)	165-202 (184)	170-205 (187)
Maximum Breadth ..	150	146	132	149	136-144 (141)	142-150 (147)	125-144 (137)	130-137 (135)	128-144 (135)	125-150 (140)	117-145 (135)	123-145 (133)	117-145 (135)	119-150 (134)
Minimum Frontal Breadth	102	108	105	105	93-108 (103)	95-100 (98)	87-100 (92)	91-97 (92)	93-98 (96)	87-108 (100)	87-105 (98)	86-96 (92)	86-105 (95)	82-114 (97)
Length, Mastoid Pro- cess	24	34	36	26					18-25 (22)	18-36 (29)	18-32 (25)	16-28 (23)	16-32 (24)	21-38 (29)

Some of these data were obtained from J. A. Keen, 'A Statistical Study of the Differences between Bantu, Hottentot and Bushman Skulls,' *Soölogiese Navorsing van die Nasionale Museum*, 1947, pp. 191-199. Mean values are given in brackets. All measurements are in millimetres.



and non-metrical features which have been analysed and included as characteristics of the Boskop 'race' are even more fallacious.

This brief preliminary table also indicates how the range of the 'Boskopoids' is almost identical with that of the Bush-Hottentots and Hottentots examined.

In this report the 'Boskopoid' skulls cannot each be discussed in detail, but doubt must be cast on their status. Comments on a few only will be given here.

Tobias (1955) regards the Plettenberg Bay skull as the best example of the survival of the Boskop type into the Later Stone Age and historic period and calls it 'a gigantopædomorph which probably became largely blended into the Bush type as the hybrid Bush-Boskop and Boskop-Bush people, through which intermediaries a Boskop element became integral part of the Hottentots.' In actual fact the reconstruction shows gross distortion and if one reconstructs the posterior half of the cranium it becomes practically identical with many of the 15 Bush-Hottentot skulls which I have collected from the Plettenberg Bay area; it also falls into the range of most of the 13 crania from the Matjes River Cave, which is actually less than four miles from the site of discovery of the Plettenberg Bay Skull.

The Cape Flats Skull has been described (Drennan, 1929) as an Australoid variant of the Boskop Race (whatever that may signify, for it is not at all like the Australian type defined by Krogman, 1932). Discovered with a typical Bushman skull and Bushman artifacts in a sand quarry on the Cape Flats (10 miles from Fish Hoek), it is probably a Negro-Hottentot specimen.

Skull U.C.T. 80 was recovered from our dissecting room in 1925. Abnormally big and heavy-looking, it belongs to a Cape Coloured. The ancestry of the Cape Coloured who have 'evolved' during the last 300 years is essentially Hottentot and European.

The Algiers Committee (1953) even include as 'Boskopoid' the two Bush-Hottentot (so-called 'Strandloper') skeletons (Keen, 1942) with their associated ostrich-shell beads found at a higher level than the Fish Hoek skull in the Skildergat rock shelter, which has an uneven, sloping, boulder-strewn floor. It is here considered that the Fish Hoek skull (and skeleton) and the distinctive Bush-Hottentot skeletons found apparently at a slightly more superficial level are essentially of the same stock. The Fish Hoek skull contains certain features absent in the others which may be due to inbreeding in a relatively small earlier, more isolated group. This may also explain why at both Zitzikama and Matjes River there are distinct Bush and various 'mixed' types (classified by the Algiers Committee as 'Boskopoid'), those of the lowest and highest levels apparently differing rather considerably. Further studies will need to identify probable dominant and recessive features and degrees of expressivity. Genetic drift also played a role in establishing differences between separated small groups originally from the same (or similar) parent stock. In similar vein, Abbie recently stated, in a personal communication, that the different skull forms in different parts of Australia have given rise to a number of views on

their different ethnological affinities, but he believes that there has only been one kind of aboriginal and these differences may be attributed to inbreeding in relatively small isolated groups.

It has only been possible to cover the periphery of this enormous problem, but it may already be concluded that further references or allusion to a Boskop 'race' should be guarded against as there is no justification or value in artificially evolving or maintaining such a 'race.' 'Boskop' should refer only to the type specimen—a large calvarium of Bush-Hottentot nature found on a farm near Boskop in the Transvaal. Further attempts to discern 'Boskop features' in the faces of living Bantu (Musiker, 1954; Galloway, 1937b; Dart, 1938) should be regarded as quite unjustified. If there was a big-headed small-faced group in African prehistory, it should, at the moment, be considered in terms of Bushman, Hottentot and Negro, for no other definite immediate ancestral types are known, except the Florisbad, Saldanha and Rhodesian skulls which, in our present state of knowledge, appear to defy description along such lines.

It is now obvious that what was justifiable speculation (because of paucity of data) in 1923, and was apparent as speculation in 1947, is inexcusable to maintain in 1958.

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#### Note

<sup>1</sup> After I read this paper in Philadelphia, Dr. L. H. Wells kindly drew my attention to the fact that I had omitted to mention the paper by T. F. Dreyer, A. J. D. Meiring and A. C. Hoffman, 'A Comparison of the Boskop with other Abnormal Skull Forms from South Africa,' *Zeits. für Rassenkunde*, Vol. VII, Part 3 (1938), pp. 289-296, wherein they expressed extreme scepticism about the occurrence of the Boskop 'race.' It may be that their paper was overlooked subsequently (as they have rarely been quoted) because their arguments were rather confusing and defective in parts although their conclusions were sound.

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## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**The Group as the Unit of Social Evolution.** By Dr. Margaret Mead. Summary of a communication to the Institute, 20 January, 1958

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The renewed interest in cultural evolution presents us with the problem of selecting the appropriate unit for the study of the process. Historically, the units chosen have been dependent upon the subject matter under discussion, e.g. development of systems of irrigation or designs on bark belts or Eskimo needle cases. Meanwhile, it has been customary in historical studies to use individual great men as markers of significant change. Julian Huxley, relying on Kroeber's type of discussion of the unimportance of individuals, still conceded some influence to individuals of the stature of Leonardo Da Vinci!

The following discussion is based upon my own fieldwork, where I have always specified every individual in every household within the closed community or communities where I did my fieldwork, but particularly on my restudy of the Manus village of Peri, in the Admiralty Islands, in 1953 and the work of my associates, Theodore and Lenora Schwartz, in the adjacent composite village of Bunai on the Paliau movement and the nativistic cult which accompanies it. From this work it seems that the most useful unit of cultural change—whether that change is evolutionary in the narrower sense or not—is not the individual, nor the trait, but a group of individuals interacting with each

other, in which both the gifted—after whom a significant change is likely to be named—and also the stubborn, the stupid, the unimaginatively executive—play a role. A detailed study of the composition of such groups, the exact nature of the network, the properties of each individual and the situation of interaction gives us the kind of detail necessary to answer such questions as the conditions under which a nativistic movement appears in one tribe and not in another, and succeeds in one place and not in another.

The recent book, *The Trumpet Shall Sound*, by Peter Worsley is an excellent statement of the wider conditions within which a rash of nativistic cults appear; but, like the theories of Marx on which it is based, it has no theory of the detailed relationships through which specific individuals—in one place and not in another—become the executors of a change which is historically based on a broad socio-economic situation. The introduction of details of specific small human groups—as the mediators of change—preserves the link between man's hereditary constitution and the changes which occur in his culture, and so makes it possible to place cultural evolution in the continuum of biological evolution without invoking a complete break between biological and cultural evolution which a simple socio-economic or super-organic interpretation of history necessitates.

## SHORTER NOTE

**The Godhardunneh Cave Decorations of North-Eastern Somaliland.** By Dr. I. M. Lewis, University College of Rhodesia and Nyasaland. With three text figures

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Godhardunneh (literally, 'the decorated hole') is a cave some eight miles to the west of 'El Afweyn ('the well with the large mouth'), a village and watering place on the present (1957) road from Bruao to Erigavo in the east of the British Somaliland Protectorate. The cave is a rock fissure in arid gypseous country. There are two mouths and the cave extends for some few hundred feet between them. The pictures here illustrated are on the steep rock faces at the southern mouth of the cave. Artificial

light was not needed to photograph the drawings. The pictures have been made by percussion on the gypseous walls some six feet or so from the ground and extending for perhaps 20 feet on each side of the entrance. Most of the figures are 12 or more inches high and from six to eight inches in width. The commonest figures are camels with very large humps which may indicate loaded burden camels or, less probably, pregnant she-camels. The large humps might also be interpreted as being exaggerated to represent camels in the peak of condition when grazing on fertile rainy-season pasture. The long trains of camels depicted in fig. 1 are particularly striking. Amongst other animals which are identifiable the com-





FIG. 1. THE ROCK PICTURES OF GODHARDUNNEH

Photographs: I. M. Lewis

monest seems to be the leopard or lion (fig. 2). Human figures, most of them apparently males and some with enlarged genitals, are common and not very skilfully depicted.

During a short visit to the cave<sup>1</sup> I could find nothing which suggested the age of the drawings. A brief examination of the boulder-strewn rock floor of the cave revealed no traces of any industry, bones or other remnants. Local Somali (Habar Tol Ja'lo)<sup>2</sup> opinion holds that this and other similar caves in Northern Somaliland were formerly used as refuges and shelters, as some certainly still are today. Tradition associates the cave with the time of the Dir, the earliest Somali clan, and the advent of the immigrant Arabian founders of later Somali clans and clan families.<sup>3</sup> My informants also hazarded the opinion that Somali sometimes took refuge in the caves from marauding Abyssinian armies during the mediaeval struggle between the Arabian-



FIG. 2. DETAIL OF FIG. 1.



FIG. 3. DETAIL OF FIG. 1.

exposed situation of the drawings on a gypsum base makes it unlikely that they are of great antiquity. Any assignment of a date to the drawings will have to await archaeological exploration. As far as I know, the only other cave drawings reported from Northern Somaliland are those discovered by Burkitt and Glover<sup>5</sup> in a cave at Tug Gerbakele near Gaan Libah in the centre of the British Protectorate, but these are less extensive and of cruder execution than those reported here.

## Notes

<sup>1</sup> My attention was first drawn to the cave by Sir T. O. Pike, Governor of British Somaliland, who with his wife had visited Godhardunneh and noted the drawings. I visited the cave during a visit to the Eastern Protectorate while engaged on a sociological study of the Somali financed by the Colonial Social Science Research Council, London.

<sup>2</sup> A large clan of the Ishaq clan family inhabiting the centre and east of the British Protectorate. See I. M. Lewis, *Peoples of the Horn of Africa*, International African Institute, London, 1955, pp. 23-5.

<sup>3</sup> For a brief sketch of Somali population movements, see Lewis, *op. cit.*, pp. 45-9; and 'Population Movements in Somaliland: Current and Projected Research,' communicated to the Second Conference on African History and Archaeology, School of Oriental and African Studies, London, 1957.

<sup>4</sup> See Lewis, *op. cit.*

<sup>5</sup> M. C. Burkitt and P. E. Glover, 'Prehistoric Investigations in British Somaliland,' *Proc. Prehist. Soc.*, New Series, Vol. XII.

## CORRESPONDENCE

Numbers in Northern Rhodesia. Cf. MAN, 1957, 141, 226

**235** SIR,—A form of enumeration similar to that described by Dr. Colson for the Valley Tonga is also sung by Lunda and Luvale children. The numbers used in these two cases are as follows:

No.	Lunda	Luvale
1.	<i>kashe</i>	<i>kashi</i>
2.	<i>kaluwe</i>	<i>kaliwa</i>
3.	<i>ketekete</i>	<i>mucheketeke</i>
4.	<i>chambambe</i>	<i>chambamba</i>

5.	<i>kukwe</i>	<i>mukukwe</i>
6.	<i>pelewenje</i>	<i>peleveche</i>
7.	<i>kalongolongo</i>	<i>kalongolongo</i>
8.	<i>kashinye</i>	<i>kashinya</i>
9.	<i>shinyangele</i>	<i>shinyangilye</i>
10.	<i>kumelyo</i>	<i>kumyolyo</i>

The Lunda and Luvale normal enumeration is, like that of the Tonga, a quinary system, and bears no resemblance to the numerals in the songs. Except that most Lunda and Luvale seem to be sure



about the order of their numerals in the song, there is little to add to their use to supplement Dr. Colson's observations on the Tonga. The Luchazi also have a similar series, closely resembling that quoted for the Luvale but with three of the numbers different. The Nkoya have a series with different terms; I have not at the moment an accurate text of these. The Dabida in the Taita Hills in Kenya evidently do the same, as shown by J. Williamson in *African Studies*, Vol. II, No. 4 (1943), pp. 215f. All these numbers appear to share the common feature of being invariables without any concordial

agreement with a noun to which they refer. This is a feature also found in some non-quinary normal Bantu systems of enumeration, e.g. in Chokwe for seven to nine and in Mbundu for six to nine. The Mbundu numerals in these cases appear to be nouns with prefix *e-*. Those in Chokwe may also be, but as prefixes have been widely lost in nouns in that language, no obvious morphological indication is present. The numerals in the Lunda and Luvale and the valley Tonga songs appear likewise to be nouns.

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C. M. N. WHITE

## REVIEWS

### GENERAL

**The Autobiography of Charles Darwin, 1809-1882.** Edited by Nora Barlow. London (Collins), 1958. Pp. 253. Price 16s.

**236** Charles Darwin. By Sir Gavin de Beer. London (O.U.P. for British Academy), 1958. Pp. 23. Price 3s. 6d.

Charles Darwin and Alfred Russell Wallace started the great advance of thought about evolution one hundred years ago, as quiet scientific workers of integrity in thought based on the penetrating observation that their simplicity promoted. The *Autobiography* of Darwin is now given in full for the first time including the details of Samuel Butler's folly which would otherwise have been appropriately forgotten. As one reads of Darwin's thoughts on his studies of coral reefs, earthworms, traces of past glaciations and many other incidental topics, and Sir Gavin de Beer's survey of Darwin's work, one is impressed with the subsequent verification of so many of his inferences by later workers. And the main thesis, natural selection as one of the factors of evolution, has been strengthened, as Sir Gavin de Beer points out, in our generation by Sir R. A. Fisher, Professor T. Dobzhansky and Sir Julian Huxley. It is regrettable that, though Mendel knew of Darwin's work, Darwin did not know of Mendel's. What biometrical aridity might have been spared us had they collaborated! The more personal aspects of the *Autobiography* include much that has long been well known with Emma as the perfect nurse of Charles the perfect patient, but we are now given Darwin's gentle references to religious matters excluded from early editions lest they should shock people. Truly the climate of opinion has improved and old storms of prejudice are subsiding.

H. J. FLEURE

**Formation et Transformation des Races.** By Guy Dingemans. Paris (Colin), 1956. Pp. 343.

**237** This book forms part of the personal testament of a religious, kindly and very learned man, profoundly conscious of what he regards as the tragedy of human history, who is under a compulsion to find, and to communicate to others, a comprehensive and satisfying explanation of the whole of existence.

The book falls into two parts, dealing respectively with the creation and with the differentiation of the human species in terms which are superficially those of genetics and, more especially, of blood-group genetics.

In dealing with the origin of man the author tries to reconcile the biblical story with the facts of geology and anthropology. He admits that man is descended from simian and pre-simian ancestors and has existed since early Pleistocene times, but insists that, as related in the book of Genesis, man as such arose once for all in a single family. Having posed the problem in these uncompromising terms the author is driven to deducing that man arose from ape by means of a single 'fertilizing' mutation on the sex chromosome X, followed by close inbreeding. This solution, quite unacceptable to most biologists, is put forward with every appearance of confidence or even dogmatism. The abundant sketches which illustrate it have, however, a pleasantly humorous quality which suggests an open-mindedness that appears only infrequently in the text.

In the second part of the book the author uses his very considerable knowledge of anthropology and genetics to put forward detailed theories regarding the origins of the subdivisions of mankind, by descent and mixing, from older races and ultimately from the types

recognized in palaeolithic times. Much that he writes is correct, or at least highly probable, but he insists on filling gaps in positive knowledge with speculation, without always making clear which is which. Moreover he treats the laws of Mendelian inheritance as a set of rules laid down once for all, rather than as being not only the main mechanism of evolution but themselves a product of evolution. He admits that gene frequencies may change through the 'Wright effect' of genetic drift, and he tilts against the windmill of Lamarckism, but he ignores almost completely the theory of natural selection and its interpretation in genetical terms. This is perhaps not surprising in one who regards the whole process of organic evolution, from virus particle to man, as one of degeneration. The book, while it will be of considerable interest to many anthropologists, must therefore be regarded as a personal document rather than a scientific guide.

A. E. MOURANT

**The Life of Mammals.** By J. Z. Young. Oxford (Clarendon Press), 1957. Pp. 820. Price £4 4s.

**238** Like T. H. Huxley long ago, Professor Young is a biological engineer, but now with the help of biophysics, biochemistry and the electron microscope. He describes activities of living beings and factors promoting the steady state (of balance rather than fixity), an attitude more fruitful than that of studying separate organs with separate functions; indeed, the author might well have gone farther still in this direction.

Homothermy promotes storing of continuous and complex memories, and Young traces the evolution of constancy of temperature among mammals, with due reference to the anomaly of the bats. Hair at first helped to preserve internal heat, and, by trapping air, partly insulated the body from a cool environment. African elephants with their low ratio of surface to volume need hair less, and may increase cooling by raising their ears and thus making their surface 16 per cent. greater. Races of hares have longer ears in warmer areas in North America. Thickness of hide, sweat glands and dermal blood vessels and fat also affect temperature control. One would have welcomed a paragraph about bodily temperature controls in human groups.

Connective tissue, with many cell types, is often protective lining and filling and is usually permeable by fluids. After a wound, randomly oriented fibres form in the blood clot, but healing causes these to disappear leaving only oriented fibres. Fat reserves, antibodies and pigment cells occur in connective tissue.

Sympathetic post-ganglionic nerve fibres secrete a fluid near adrenaline close to the muscles and glands which they innervate, a survival of the wide distribution of adrenal tissue in early chordates. Nearly all endocrine organs have evolved from organs which had more direct relations with the exterior. Adrenaline contracts arterioles of skin and of most viscera, but dilates those of heart, lung bronchioles and most muscles, contracts however muscles of the pregnant uterus. The thyroid stores and secretes iodine, yet injection of iodine may relieve hyperthyroidic trouble. These facts show that we must be cautious in describing hyperstatic and hypostatic conditions, anaboly and cataboly and so on. Young thinks that we too readily assume the disappearance of the thymus in adult life. Disease stress destroys the thymus and this may account for its common absence from corpses. Excitation of gonads and adrenals



also promotes decline of the thymus. Hormones from endocrines and other organs are distance signals, often but not always transported by the blood stream.

The cerebral cortex receives signals from more and more receptors the higher the mammals studied. It stores patterns of associated signals as representations of the environment useful in computing outputs that promote anticipatory actions adequate to maintain life. It works in close linkage with the hypothalamus which integrates visceral and somatic activities with the help of mid- and hind-brain and spinal cord.

Receptors send continuous impulses into the nervous system; stimuli modify but do not initiate them. The embryo is bisexual at an early stage, but hormones from the gonads cause disappearance of the Wolffian (male) or Mullerian (female) ducts respectively. These facts help in interpretation of abnormalities.

H. J. FLEURE

**The Evolution of Living Things.** By H. Graham Cannon. Manchester (U.P.), 1958. Pp. x, 180. Price 12s. 6d.

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In this short book, written for laymen and for workers in other branches of science, Professor Cannon develops his arguments against the neo-Mendelian interpretation of the mechanism of evolution. Like many naturalists he feels that the amazing variety and intricacy of adaptation and regulation which living things display is something that could not have been produced by natural selection acting on undirected mutations. He prefers an explanation, if such it can be called, in terms of some ill-defined directive principle. He is sympathetic to the views of Lamarck which he believes to have been misinterpreted both by his contemporaries and by later biologists. He gives brief accounts of the work and views of Darwin, Lamarck and Mendel. It is often the case in polemical writings that the author seems to be insufficiently informed about the field he is attacking. One wonders whether Professor Cannon has studied the writings of Fisher, Haldane and Wright in which problems of evolution are dealt with not by instinctive belief or disbelief, but by setting up models and considering their implication in quantitative terms. He appears to think that Galton's demonstration of regression to the mean when the correlation between fathers' and sons' stature is examined implies that stature is not inherited; this is a complete misunderstanding. He seems also to be handicapped by too rigid a view of the relation between a gene and the 'character' which is the end result of gene action, and so does not appreciate the numerous and subtle ways in which indirect and unexpected effects of gene action may result from interference with some developmental process. He states that the ability to maintain a high body temperature is an outstanding mammalian characteristic and finds it hard to envisage such a major step arising by a neo-Mendelian mechanism; however, mammals vary considerably in their capacity for temperature regulation and it is, therefore, not too difficult to conceive many stages in the evolution of this attribute. He also seems to think that viruses and bacteria cannot depend much on genes because they have no chromosomes; however, the modern evidence for the linear arrangement of genes on some structure analogous to a chromosome in these organisms is quite strong.

It is in some ways stimulating to read a book which attacks widely held views, and it is easy to agree that our explanations of evolution remain very incomplete and imperfect. Nevertheless, Professor Cannon does not tell us how to set about investigating his 'directive principle' or where we may find a good substitute for a field of genetical research which is very much alive and seems to have excellent prospects.

N. A. BARNICOT

**Manual de Antropología física.** By Juan Comas. Mexico and Buenos Aires (Fondo de Cultura Económica), 1957. Pp. 698, 114 text figs.

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The author of this book, Dr. Juan Comas, has been attached for many years to the Universidad Nacional Autónoma de México, in which he has been responsible for the direction of studies in physical anthropology. His intention in writing this book was to provide a suitable Spanish elementary text for beginners in the subject. In this he has succeeded quite well. There are ten

sections in the manual. The first consists of a historical introduction which contains the usual ritual glorifications of the Greeks, deprecation of the mediaevals, and admiration of all who followed them. This is followed by a well done introduction to statistical methods by Felipe Montemayor, which would have been up to date in 1908, for they are entirely Pearsonian. Evolution, heredity, growth, blood-grouping and constitutional typing are covered in the next five sections. Cranial and general osteology follow; and chapters on paleo-anthropology and racial grouping complete the strictly pedagogical portion. The book ends with a disquisition on the uses of physical anthropology in other fields of life and learning. There is a five-page bibliography of standard works and a very useful three-page list of anthropological journals. There are two indices, one by subject and the other by personal names.

Some parts of the book have an emotional tone which would be unusual today in similar works issued in Western Europe or north of the Rio Grande. The author is clearly perturbed about the Nazi regime and about the corresponding prejudices against Mestizos, Negroes and Jews which he finds in the New World. He is so thoroughly imbued with generosity of spirit in this matter that he permits himself to assert that the notion of a Jewish race is wholly fictitious, and fails to distinguish between the Sephardim on the one hand and the Ashkenazi on the other, spoiling a good thesis by stretching it to absurdity. His wide reading has not, apparently, extended to Gunnar Dahlberg's *Arv och Ras* (1940) where the matter is treated with admirable dispassion. It is also noteworthy that he introduces the topic of evolution even before that of heredity, and that this latter subject is presented as if Neo-Mendelism covered the whole field of the phenomena. It is doubtful whether an axiom system should be presented so early in a book essentially devoted to the methods of an empirical science. Nevertheless, the general plan and presentation of the book is worthy of copy over here.

M. A. MACCONAILL

**Human Craniology: A Somatomorphological Interpretation of the Human Cranium.** By Dr. Bruno Oettking.

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New York (Chiropractic Inst.), 1957. Pp. 144

This is a handbook largely devoted to the detailed terminology of cranial anatomy, and it will be useful for quick reference purposes concerning minor anatomical features which are not usually included in the standard anatomical textbooks and atlases.

The relevance of the subtitle is not altogether clear. It seems doubtful if the cranium can be treated adequately by itself in an introductory book without including much material covered by such works as *The Tissues of the Body* (Le Gros Clark, Oxford, fifth edition, 1958) or by Mitchell and Patterson's more elementary *Basic Anatomy* (London, 1954). Dr. Oettking's short sections of explanatory matter are very greatly oversimplified. There is little in this handbook to promote an understanding of bone structure or of the many factors which influence cranial growth. The developmental process from birth to adulthood is scarcely mentioned.

There is a careful if pedantic index. The list of 'Literature Consulted' would have been much more useful expanded into a short, up-to-date bibliography. The only illustrations take the form of line drawings which are clear so far as they go.

P. M. DANBY

**Function, Purpose and Powers: Some Concepts in the Study of Individuals and Societies.** By Dorothy Emmet.

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London (Macmillan), 1958. Pp. vii, 300. Price £1 8s.

This is a theoretical study of major importance for social anthropologists. In it a professional philosopher who is well read in social anthropology examines and develops some of the most important concepts used in the latter discipline. Some idea of the quality of Professor Emmet's thinking and of the relevance of her work for anthropologists may appear from a brief summary.

After setting out her aims she proceeds in Chapter II to analyse the concept 'society.' She has no difficulty in showing that its use in social science has oscillated between reference to a concrete group ('a number of people are somehow together'), an entity which she usefully designates 'social aggregate,' and reference to a social



structure of some kind, that is, to 'an abstraction based on certain selected types of relationship.' Not, be it noted, on all the kinds of social relationships which constitute the social aggregate; the concept so broadly interpreted is unusable, as Radcliffe-Brown failed to see. Professor Emmet perceives that social structure is neither an empirical entity nor a subjective whimsy, but a construct deriving both from the data and from our particular interest in it: on this point Nadel and Lévi-Strauss are both right—and both wrong. Professor Emmet does not, however, draw the logical conclusion that different social structures may be elicited from the same social aggregate, depending on the nature of the analyst's interest in it; she prefers to use Fortes's concept of social 'fields' (or, better, 'constellations') for the particular complexes which may be abstracted. In the next chapter the concept of function is analysed, and the important point is made (among others) that if function is to mean more than merely effect it must have reference to some kind of enduring totality, the efficiency or maintenance of which can be shown to depend on whatever is said to have a function. It might perhaps have been made a little clearer in this chapter and the next that what is required to make the notion of function intelligible is the idea of a conceptual totality, not necessarily of an empirical one; functionalism need not involve holism in the sense in which Professor Popper has so properly and so devastatingly criticized it (though it has sometimes done so). In Chapter IV Professor Emmet turns to the more specialized concept of *social* function, developing the idea that to be meaningful it must imply an 'ordered context,' and aptly proving her point by reference to Evans-Pritchard's account of Nuer blood feud.

Chapter V discusses 'purpose' (different of course from function, since purposes are done 'on purpose'), and pleads that students of society be 'prepared on occasions unashamedly to adopt the language of purpose and intention,' since, as Nadel and others have stressed, sociologically relevant behaviour is always (among other things) purposive. In fact, of course, social anthropologists have always described 'purposes,' i.e. what people think they are doing, and they have thus been led to the study of beliefs and values. But in their anxiety to be 'scientific' (i.e. non-teleological) they have not always been clearly aware of this. Chapter VI gives an account of open and closed moralities, contains some legitimate criticism of Professor Popper's well-known ideas on these topics, and takes up the question of the ways in which exceptional people may, through 'vocation' (that is, a particular kind of creativeness) affect their societies. Chapter VII interestingly and provocatively discusses the functions of ritual, though Professor Emmet's discussion of taboo and totemism (which she apparently supposes to be confined to plants and animals) is weakened by her lack of acquaintance with recent work on these topics. The next chapter deals with charismatic and other kinds of personal power, especially those expressed in blessing and cursing, and in the notions of holiness, *mana* and prophecy. This chapter is particularly rich in stimulating and original ideas. The central theme of the book, which is that the structural-functional approach to the study of social aggregates misses a great deal of interesting material regarding the place of the individual in society, now begins to become explicit. It is further stressed in Chapter IX, which deals with the problem of individual creativeness or 'vocation.' The final chapter is a review and reassessment, and reasserts Professor Emmet's contention that a society is not usefully regarded merely as a mechanical system of checks and balances operating automatically, but is rather a process, the elements of which are 'mobile individuals with private purposes, conflicts and allegiances.' To disregard these, it is implied, is gravely to impoverish the study of society.

This brief summary does less than justice to Professor Emmet's lucid and important study. No social anthropologist can afford to ignore it. But, having said this, I must ask why it is that the second half of the book seems somehow less satisfactory than the first half, most notably the brilliant chapters on society and function. This may well be partly because the later chapters deal for the most part with concepts which fall outside the familiar and orthodox theoretical armoury of social anthropology. But it is not only this. As the book develops, the suspicion grows that despite the distinction indicated in her sub-title, the author has not quite made up her

mind whether the proper study of social anthropologists is social institutions, or people, or both. Most modern social anthropologists suppose themselves to be studying social institutions, such as law, kinship systems, ritual cults, systems of exchange, and so on, and they quite explicitly abstract these institutions not only from the individuals who participate in them, but also, to a large extent, from the manner in which they came to be what they are. It is in this 'institutional' frame of reference that analyses in terms of such notions as social structure, function and, to some degree, purpose, are relevant. One may, for example, sensibly ask what is the function of an institution (such as the Nuer blood feud), but it is not useful to ask what are the functions of particular individuals (as Professor Emmet sometimes—e.g. on p. 105—seems to be doing), however creative these individuals may be. Conversely, problems of individual creativeness must concern the student of men in society equally with the historian of a particular society; they are less relevant to the study of such institutions as the blood feud, or bridewealth, from the point of view of their social implications. Thus there are evidently two quite different orders of problem there, implying two quite different levels of analysis, and it is not always entirely clear when Professor Emmet is operating on the level of institutional analysis, and when on that of individual personality study. It must be said, however, that this confusion (in so far as it exists, and is not merely a projection of my own uneasiness!) reflects a confusion in the fabric of social anthropology itself. It is true (in a sense) that the material of social anthropology is in the last resort people, and it is certain that our interest in people is by no means exhausted when we have submitted their institutions to functional analysis. The point is one which is eminently worth making, and sooner or later social anthropologists will have to decide what they are going to do about it. In its original presentation of this problem Professor Emmet's book is at once a stimulus and a challenge to every serious anthropologist.

JOHN BEATTIE

**Family and Social Network: Roles, Norms and External Relationships in Ordinary Urban Families.** By Elizabeth Bott. London (Tavistock Publications), 1957. Pp. 252

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This is the first of two books to be published by the Tavistock Institute on their intensive, interdisciplinary study of a small series of ordinary—not 'problem'—London families.

Urban families not in trouble have not been sufficiently studied and are difficult to engage in long-term observational studies. Furthermore, little is known about the ways in which they delineate their significant social environments within the urban setting. In this context, the rather cumbersome methodological chapter by Dr. Bott and Dr. James Robb (who did much of the field work with her) is more than justified.

In a chapter on 'conjugal roles and social networks,' Dr. Bott sets up her key concepts, and makes perhaps her most stimulating set of contributions. The families studied varied in internal organization from those where husbands and wives performed their role functions 'jointly' (together or interchangeably) to those in which role functions were comparatively 'segregated.' With regard to external relationships, Dr. Bott notes, following Dr. John Barnes, that urban families do not live in 'organized groups' but in 'social networks'—i.e. collections of individuals who interact with them and with one another to form a more or less reticulate pattern of relationships. 'Close-knit' networks have many such interconnections among members, while 'loose-knit' networks have fewer.

It was found that 'the degree of segregation in role-relationships of husband and wife varies directly with the connectedness of the family's social network.' Dr. Bott advances this finding as a hypothesis for further extensive investigation.

The existing literature on families tends to provide specifications for one or other of these dimensions of family life, but the way Dr. Bott has linked them is novel. Within the limits of her own small series, she makes some preliminary assessments of factors possibly associated with the different patterns of social networks. Using a configurational approach, she finds that close-knit networks are associated with long-established, homogeneous, working-class



neighbourhoods, proximity of kin, and stability of residence. Dr. Bott's chapter on relations with kin contributes not only to the understanding of network factors but to the growing literature on kinship in British society. For each of these factors association with a network pattern is a complex one, contingent on other factors.

A more critical set of variables is developed around the concept of norm, with which Dr. Bott links psychological and sociological determinants of network-formation. Norms are defined as 'people's ideas about what behaviour is customary and what behaviour is right and proper in their social circles.' Norms are learned by individuals not as a simple matter of passive internalization, but by a continual process of introjection and projection, interspersed with the work of personal adjustment. Validation of norms is facilitated in close-knit networks, while loose-knit networks seem to foster the development of abstract reference groups as the loci of normative standards. Dr. Bott thus contributes important qualifications to the old maxims of W. I. Thomas and of the psycho-analysts, that a social situation if defined as real is real in its consequences. She is concerned, conceptually, with both the actually prevailing set of norms ('norms of common consent') and the norms which an individual *perceives* to prevail in his social group ('social norms') or to be uniquely his own ('personal norms').

Dr. Bott's book represents exploratory research at its best. Systematic definition of key concepts and rigorous adherence to them in analysing the data are achieved even though the entire framework is so new as to be emerging in the course of the study itself. While I have some reservations about Dr. Bott's choice of conceptual terminology, her approach constitutes a decided advance. The network concept offers a dimension intermediate between relationship and social system. It is at once broader and narrower than kinship analysis, and more relevant to the task of conceptualizing people's ways of defining, circumscribing and populating their sub-universes within the urban setting. As the variables for analysing social networks are more fully developed and refined, the concept is likely to become increasingly valuable for a variety of types of social research.

ROBERT N. RAPOPORT

**Common Frontiers of the Social Sciences.** Edited by Mirra Komarovsky. Glencoe, Ill. (Free Press) and New York (Falcon's Wing Press), 1957. Pp. vii, 440. Price \$6

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Analytical procedures evolved by sociologists steer them towards collecting certain types of facts within their field of interest. In this book this approach is used on problems typical of those that attract historians and economists. In her introduction the editor gives a very valuable summary of the possibilities which these new techniques and new sources of data offer.

The two principal sections into which the text is divided, History and Social Research, and Economics and Sociology consist of specific frontier problems tackled first by a sociologist and then by a historian or an economist, as the case may be. For the anthropologist, the analysis of the divergence between public opinion polls and traditional data about political values in France between 1944 and 1949 and secondly the defence of an anthropological approach to the study of the factory by Conrad Arensberg and Geoffrey Tootell will have most interest. Other writers show how survey research can be used to test and to modify Keynesian economic theory and how reference-group theory may be used to explain wage policy in American trade unions.

This treatise illuminates many scattered research trends in the social sciences and is a worthy complement to books that have already appeared examining the frontier shared by anthropology, social psychology and sociology. It will be indispensable for those anthropologists who consider that research within modern communities, rural and urban, is a legitimate field of activity for the anthropologist.

JOHN MOGEY

**Sociology.** By Joseph H. Fichter. Chicago (U.P.) (U.K. agents: C.U.P.), 1957. Pp. xiii, 449. Price £1 17s. 6d.

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The laudable aim of this beginners' textbook is to avoid the teaching error of giving too little empirical illustration of statements of definitions. Each chapter first gives *ad hoc* generalities, then, separately, description of American society.

But surely this method both denies to the beginner the emphasis—when perhaps it would make the most impression on him—that sociology is a comparative science, and also precludes what would counterbalance this lack to some extent, a systematic presentation of the institutions of a single society. It is a general feature, however, that sociology in America is increasingly becoming sociology of America.

To what extent a student must be partially misled to be introduced to a subject is clearly a highly debatable point; thus comment on Fichter's text also in this respect involves a general issue. Nevertheless, even for the beginner it is surely needless to read—and on a single page—that 'religion is one of the major institutions which are found in every society,' and 'it is probably true that social status has always in every society been measured more on secular than sacred factors.' Also, besides the further factual dispute an anthropological sociologist would enter on the view that 'what appears to be unique in the American culture is the consideration of competition as a positive, associative form,' it is odd to find later in the book that competition is labelled simply as a disjunctive social process. A notable omission from Fichter's list of processes is superordination-subordination or hierarchy—terms also absent from the index. Could this have resulted from the lack of comparative perspective in this textbook, which is otherwise not without its uses for beginning students in providing simple statements about much of the subject matter of sociology?

R. J. APHORPE

**Social Psychology.** By W. J. H. Sprott. London (Methuen), 1956. Pp. xiv, 268. Price 21s.

246

This important book, first published in 1952, has now been reprinted with minor corrections, but unfortunately not brought up to date. Nearly all references stop at 1950, and there are very few corrections indeed. Even some of the printer's errors have been retained: for instance, Quinn McNemar is listed in the Index as P. McNeman and in the text as Q. McNeman. On the other hand, the freshness and vividness of style have been retained as well, and the book does not give the impression of being out of date at all. It is in fact very modern in outlook, sensitive to cross-fertilization of various disciplines in the field of the social sciences, comprehensive and yet not too full of details, scholarly and yet a pleasure to read. The author is interested in anthropology and devotes a substantial part of his book to the mutual influences between anthropology and social psychology. Out of 11 chapters two are devoted to the anthropological approach: one deals with cultural influences on the development of personality, the other with cultural analysis and national character. Both these chapters are stimulating to read, and controversial material is analysed with common sense and understanding. The style of reporting reveals a sense of proportion which leads directly to a sense of humour and the ability to maintain an amused detachment.

Most textbooks on social psychology have been growing in size in an obsessional search for absolute comprehensiveness in a very wide field. This book is less voluminous and can be read and enjoyed by workers in the related fields. Its clarity of exposition makes it suitable for the general public as well.

T. GRYGIER

**Nonparametric Statistics for the Behavioral Sciences.** By Sidney Siegal. New York and London (McGraw-Hill), 1956. Pp. xvii, 312. Price £2 9s.

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Probably the most powerful use to which statistical methods are put in the social sciences is in the testing of hypotheses. For example, it is frequently necessary to test the significance of the difference between two sample means by testing the hypothesis that the two samples have come from populations with the same mean. In applying the usual parametric 't' test to such a problem various strong underlying assumptions are involved. The observations must be independent; they must be drawn from normally distributed populations. The variable involved must be measurable on an interval scale.

The characteristic feature of parametric statistical tests of this kind is, then, that certain assumptions are made about the parent



population from which the sample was drawn. A non-parametric test, on the other hand, makes no assumptions about the parameters of the parent population. This does not mean that in a non-parametric test no assumption of any kind is implied. These, however, are very much weaker than those involved in the parametric tests. There are other important advantages in the non-parametric tests. They can apply to, merely, ordinal data or, even, sometimes to purely qualitative classifications. They are usually easier to understand and to apply than the parametric set. The only serious objection to non-parametric tests is that if all the assumptions of the parametric test are, indeed, met then the non-parametric method is wasteful of data.

The book under review collects together in a convenient and readable form the series of non-parametric tests which have, so far, been developed. Some of these are already well known. The accounts of others, however, with the accompanying tables of significance values have previously been available only in various scattered publications.

Non-parametric tests are of particular importance in the social sciences because it is just in these disciplines that the variables are often difficult to quantify and also because doubt exists as to the validity of the assumptions underlying parametric tests. Indeed there is difficulty in many situations in contemplating at all the idea of a population of which a particular set of observations can be regarded as a sample. The concept of probability, itself, in its normal sense, presents difficulties in the case of a single once-for-all event.

The book is eminently readable and the methods are presented clearly, according to a uniform pattern, in a way which should be easily understood by all readers. Many examples are worked through in the text but none given at the ends of chapters for the student's 'homework.' This would be useful in any future edition because one of the difficulties about this field is being sure of which is the most suitable test to apply in any given situation. Another deficiency is that the method of presentation may encourage researchers blindly to follow the steps of the calculation in some test without really understanding fully what they are doing. Certainly they will not gain a true understanding of the theory behind the method from this book. Indeed it has been suggested by the reviewer of another recent work on this subject that the mathematical development of non-parametric theory is not in a sufficiently advanced stable form for a satisfactory book to be written at the present time. While this may doubtless be true, Professor Siegal has, nevertheless, performed a useful service and a beginning has to be made. Many social scientists will be indebted to him for introducing them to what is, potentially, a most useful set of statistical tools. K. S. LOMAX

**Art in the Ice Age.** By Johannes Maringer and Hans-Georg Bandi. London (Allen & Unwin) and Basel (Holbein), 1953.

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Pp. 168. Price £2 5s.

This is an attempt to write a book to a plan suggested by Hugo Obermaier before he died in 1946. As former pupils, the authors claim an intimate knowledge of Professor Obermaier's ideas and intentions. Even so it must have been a very difficult task and they are to be congratulated on having produced such a pleasantly readable work, especially suitable for the general reader. Professor Obermaier had selected some of the illustrations and his plan seems to have been to link a general outline of European palaeolithic and East Spanish rock-shelter art—to this the authors have added the Arctic art of Northern Eurasia. The book is therefore divided into three sections, that on Palaeolithic Art by Maringer and the other two by Bandi. Had this book really been written by Obermaier it would have been a counterpart to Breuil's *Four Hundred Centuries of Cave Art* in that it would have been a final summing-up by one of the two great experts on Cave Art. It is doubtful, however, if it is quite up to this very high standard. A smaller book, it is broader in scope than Breuil's and hence much less detailed. In this breadth, the main value of the book lies, and especially in the section on the less familiar Arctic Art.

Both the bibliography and the index are too limited and the general treatment is often not nearly as thorough as one would have wished.

There is a very clear two-page map of Europe showing the location of the main European sites but it is a pity that not all the sites shown are named; none of the five sites shown in Britain is identified.

After the briefest outline of the archaeological background, there follows a section on palaeolithic art in which the 'Minor Arts' are discussed first, then painting, engraving, sculpture, art styles and the somewhat over-stressed 'Art Schools,' which seem more likely to have been the result of magico-religious ceremonies rather than the remains of Academies of Drawing! In any case much depends on the great visual memory of primitive peoples. The idea of the artist as a full-time specialist seems doubtful as does the inference that sketch sheets were always trials for large paintings. Comparison between figs. 138 and 139 is convincing, but the narrowness of the art convention within certain groups must produce frequent coincidental similarities (figs. 136 and 137). The treatment of the motivation behind all this art is disappointingly brief and a discussion of the female figurines would have been more appropriate here. An excellent feature is the juxtaposition of familiar drawings with less familiar photographs and the magnificent colour photographs—although quite a number of the illustrations are very hackneyed indeed.

Obermaier's term of 'Spanish Levant Art' is employed throughout the next section and a few coloured photographs would have been welcome instead of a preponderance of frequently published copies. The authors favour a mesolithic date with Upper Palaeolithic origins, but admit that there is a general absence of stratigraphical evidence. Since they acknowledge the connexion with Africa, it is a pity that this is not gone into more fully.

The final section comprises the Arctic Art of Northern Eurasia and unfortunately it is also the shortest (19 pages as against 96 and 28 for the Palaeolithic and Levant respectively). Discovery, meaning, technique and dating are again covered, as is their relationship to palaeolithic art. Some of the art shows a late Aurignacian style (including the meander pattern) and the authors suggest that it would be easiest to derive this from the late Aurignacian of Central Russia. This section is not so well illustrated as the first two and colour photographs only occur in the first section.

The authors are to be congratulated on a book which gives a broad picture of the palaeolithic and mesolithic art of Europe within a relatively small compass.

REAY ROBERTSON-MACKAY

**Prehistoric Men.** By Robert J. Braidwood, 3rd edn. Chicago (Nat. Hist. Mus.), 1957. Pp. 187. Price \$1.25

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The first edition was noticed in MAN, 1950, 54.

Written in a colloquial style, as if for a lecture, this is a reliable introductory work, up-to-date and carefully compressed. The latter half of the book reviews the Neolithic. Briefly, the author is sceptical of the early dates at Jericho (p. 120); he believes in an independent New World, and possibly Chinese and South-East Asian (though not, in this edition, Indian) Neolithic (p. 136); and he expounds a theory of neolithic origins in terms of 'nuclear areas' and an 'atmosphere of experimentation.' This thesis is important, in view of the wide field experience on which it is based.

W. C. BRICE

**Science in Progress: 10th Series.** Edited by H. Taylor. New Haven (Yale U.P.) (London agents: O.U.P.), 1958. Pp. 253. Price £2 12s.

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This is a series of essays by many authors on recent science. Vannevar Bush deplores tendencies in the U.S.A. to persecute science and so to deter men from fundamental research. P. B. Sears thinks the U.S.A. weak in fundamental research but strong in development and application of discoveries. Some articles emphasize observations on plants and animals bred and reared in controlled environments. Oakley and Weiner give an account of the additions to scientific method arising from detective work on the Piltdown forgery. Nutrition among underfed peoples is reviewed.

H. J. FLEURE







(a)



(b)



(c)



(a) *The potteries at Taourirt*

(b) *A Taourirt potter at his wheel*

(c) *Some of his products*

# POTTERY-MAKING AT TAOURIRT, MOROCCO

*Photographs: T. H. Beckett, 1955*



# TWO POTTERY TECHNIQUES IN MOROCCO\*

by

T. H. BECKETT

**251** This article has been prepared from notes and observations made in August, 1955, on the techniques of two small communities of potters in Morocco.

## Takouchtem

*Site and channels of communication.* This small village community of 50-70 people is situated in a dry valley on the northern slopes of the Central High Atlas mountains at a height of approximately 4,000 feet, some five kilometres east of Zerekten, which stands astride one of the three main routes over the High Atlas mountains. This route provided one of the main links between Marrakech and the cities of the north, and the south of Morocco from which caravans left to cross the Sahara to Timbuktu and the kingdoms of the Niger.

Though it occupies the northern end of the territory of the Glawa tribe, the village of Takouchtem is considered by the other communities of the tribe to be distinct from them. It enjoys considerably more independence than its neighbours, having its own council of elders, *Ait Arbain*, and an elected chairman, *Moquaddem*, to manage all questions of internal government and policy.

*General description.* Potting is the main occupation and livelihood of the community. All the village participates; as a rule the women collect the clay and prepare it, and the men perform the more highly skilled task of potting. The pots are bartered with the neighbouring communities for agricultural produce, or to middlemen who sell them at the local market (*Souk*). Pottery, however, is insufficient to maintain the whole village, and many of the young men seek work in the plains during the summer season.

Attached to each house is a shed for potting (fig. 1), equipped with one or two wheels, a bin or trough for prepared and unprepared clay, and a container (an old can, or even a hole in the ground) for water. There is little scope for refinement in the techniques used, and the only explanation that could be elicited was that the pots were made very thick to avoid breakage.

Two factors are important: first, the surrounding communities are extremely poor; secondly, these same communities have easy access to modern industrial products at the local *Souks*. The pottery is therefore strictly functional, consisting of a few highly differentiated types adapted for specific purposes. There is no question of the pottery being used solely for its decorative effect, or being retained where another article, say of metal, would do the job better. Glaze is applied only to those pots whose function requires it, and pots are either glazed or painted, but never both.

*Detailed description of specimens collected.* (a) Large water pot, reddish-buff ware with reddish slip on lower part, and a design

\* With Plate O and three text figures

of bands and chevrons reminiscent of a carrying harness; long neck and thickened rim; handle from mid neck to shoulder; height 48.5 centimetres, maximum diameter of body 23.8 cm. Carried by a cord of woven reeds tied to the handle and slung over the shoulder.

(b) Oil pot, reddish-buff ware with reddish slip on lower part; design of concentric circles and bands; long neck and slightly thickened rim; handle from middle of neck to shoulder; small foot; height 43.5 cm., maximum diameter 21.5 cm.

(c) Jug for butter or milk; reddish-buff ware, unglazed, with line of red near base and lattice pattern round rim; height 12.7 cm., diameter 16.3 cm.

(d) Circular brazier, reddish-buff ware, with two bands of moulded decoration; three supports for holding dish projecting from rim towards centre; height 13.7 cm., diameter 27.3 cm. Used with charcoal. (In many houses earthenware braziers have given way to their metal equivalents.)

(e) Shallow circular food dish with conical lid; reddish-buff ware; upper surface of body and lid highly glazed in brown; hollow ring-shaped handle on top; height with lid and handle, 14.1 cm., diameter 24.5 cm.

*Materials and preparation.* Three kinds of clay are used: reddish-buff for the body, light ochre and deep brown for slip decoration. All are taken from the sides of the valley in which the village is situated, at different heights.

The clay is chipped out, brought to the village as hard, dry lumps, beaten to a powder with a long, cylindrical pole, and then sieved. The pulverized clay is mixed by hand with water, on the floor of the shed, in any depression in the ground near at hand. There is no attempt to purify the clay, and no broken pot or other tempering material is added.

Galena, bought in hard lumps at the *Souk*, is used for the glaze. There are many sources of galena in Morocco. It is first beaten in a pestle and mortar and then ground in a simple stone quern such as is used for grinding corn.

*Manufacture.* (a) The wheel. A double wheel is used, consisting of one wheel above another on a common axis, the bottom one being kicked by the foot. It is set in a hole in the ground, with the top wheel at floor level, and the operator sits on a ledge cut in the wall of the pit



FIG. 1. SHED FOR POTTING AT TAKOUCHTEM



(cf. Plate Ob). The wheel shaft is loosely strapped to a horizontal supporting bar, fixed at both ends into the ground.

(b) Shaping. In general, the pots are thrown on the wheel, without the use of any previously prepared mould. A strip of wood is sometimes used to achieve a flatter surface. There is, however, a special technique for making the large water jars. The bottom half of the jar is thrown in the normal way, and left to harden a little in the sun. Then the top half is put on by the coiling technique, the wheel being kept turning. The neck and handle are modelled separately and joined to the jar while the clay is wet (fig. 2). Specimens were collected of the jar in the three stages of preparation.

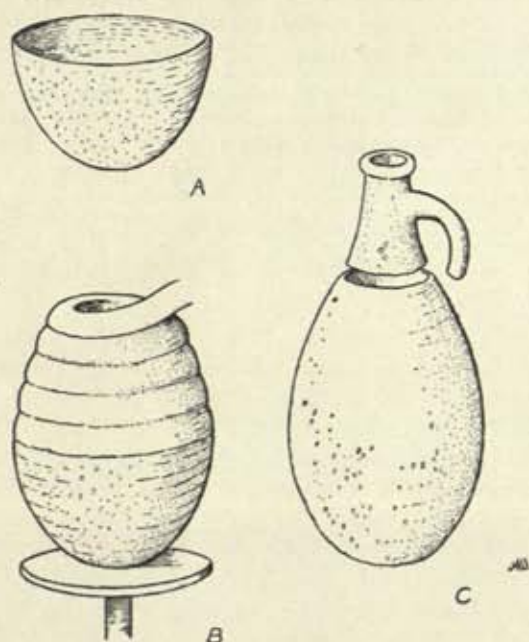


FIG. 2. STAGES IN THE MANUFACTURE OF A LARGE WATER JAR AT TAKOUCHTEM

(c) Application of slip and glazing. As previously mentioned, pots are either decorated or glazed, but never both. Slips are applied over either the whole or part of the surface, or as line decoration, with any implement that comes to hand; a few hairs twisted together, or a wad of paper, etc.

(d) Firing. Pots are left to harden in the sun before being fired. There are three kilns of a very simple type in the village. A hole is dug in the side of the slope on which the village stands, and an opening left at the top for inserting the pottery. Another opening is made at the bottom corner for stoking the fire. Local brushwood is used for fuel. The pots are placed inside the kiln, which is then roughly closed with large pieces of broken pot. The fuel is lighted, and the fire kept up for about eight hours. It is then allowed to die down, and the pottery is left for about 36 hours.

The firing is the crudest phase of the whole technique. The piled pots are separated from each other simply by a small three-pronged piece of fired clay. Glazed pots drip on to unglazed pots, and many pots stick together. In consequence, there are many flaws on the finished products.

*Technical description of pots.* The analyses given and the conclusions drawn are all those of Mr. R. D. G. Faudree of New College, Oxford.

(a) Clay body. A kaolin clay with no mica was used, producing a biscuit of reddish-buff colour. Analysis figures: 78 per cent. silica; 6 per cent. alumina; 13 per cent. iron oxide ( $\text{Fe}_2\text{O}_3$ ).

(b) Slips and decoration. Two different clays were used, one lighter and one darker than that of the body. The pigments in both cases were iron oxide, the lighter colour being an ochre of smaller particle size.

(c) Glaze. It was observed in the field that the ground galena was applied to the pots in a pure state, without the addition of a flux or of ground-up siliceous material. Chemical analysis by Hilger Spectrograph confirmed this observation, showing that a pure lead glaze had been produced, revealing no trace of alkali or lime whatsoever. This pure lead glaze (lead, 80.29 per cent.) is formed by the combination of the galena applied to the surface of the pottery with the silica of the clay body. This glaze would have been formed at  $750^\circ\text{--}850^\circ\text{C}$ .

(d) Firing. X-ray powder photographs of the clay of one specimen of the pottery showed a weak Tridymite pattern. Quartz changes into Tridymite at  $870^\circ\text{C}$ . It was thought, therefore, that the pottery was fired at approximately  $850^\circ\text{--}870^\circ\text{C}$ . Such a temperature is very easily achieved with a simple firing method.

At no time was the kiln at all airtight, therefore the pottery was not fired in a reducing atmosphere, nor discoloured in firing.

*Conclusion.* The points of interest about this technique of pottery-making are: (a) the special method of making the large water jar; (b) from a technical point of view, the use of a pure lead glaze, obtained from the combination of galena with the silica of the clay surface to which it is applied.

#### Taurirt

*Site and channels of communication.* This small community of potters (Plate Oa) works just beneath the walls of the Kasbah of Taurirt, which stands at the edge of the town of Ouarzazate, at the foot of the Central High Atlas, on the southern side, just north of the junction of the great valleys of O. Draa and O. Dades. The town of Ouarzazate contained the chief administrative centre of the *Cercle*, and stands at a main road junction. The former French administration, however, followed the policy of protecting the integrity of indigenous industries and it is probable, therefore, that this particular technique has been little affected by its close contact with modern developments.

*General description.* The products of the pottery are sold at the local *souks* and to a few passing tourists. The potters make much the same variety of shapes as at Takouchtem; there are, however, a few more highly specialized forms. These include pipes of perhaps 60–100 cm. diameter, to be used for chimneys, and pedestal oil lamps. As at Takouchtem, pots are either glazed or decorated with slip (cf. Plate Oc).

*Detailed description of specimens collected.* (a) Jar of reddish ware, whitened on the surface (see below—Firing); designs, spots, rings, etc., of a grey-brown slip; handle from neck to shoulder; neck tapering to rim; height 25.9 cm., maximum diameter of body 13.9 cm. diameter at rim, 3.7 cm.

(b) Circular bowls; reddish ware, whitened on the surface; rim and designs of lines and dots inside each bowl, differing in detail; flat base and small handles; height 5.9 cm., diameter 13.5 cm. (with slight variations).



(c) Nearly globular vessels with spout; handle in plane of rim, and closely fitting lid which covers spout also; reddish ware, whitened on the surface; pattern of lines and triangles of lattice work on lid and double line round rim, in grey-brown. Total height (including knob on lid) 21.5 cm., maximum diameter of body (excluding handle and spout) 22.1 cm.

(d) Deep circular bowl with seven large holes in the base, reddish ware, whitened on the surface; no decoration; height 17.0 cm. maximum diameter (at rim) 23.0 cm. Used for making *kous-kous*. The method of use as I observed it is precisely the same as that described by Mungo Park (*Travels in the Interior Districts of Africa*, 1795-97, Chapter I):

'The corn thus freed from the husk is . . . beaten into meal, which is dressed variously in different countries; but the most common preparation of it, among the nations of the Gambia, is a sort of pudding, which they call *kouskous*. It is made by first moistening the flour with water, and then stirring and shaking it about in a large calabash or gourd, till it adheres together in small granules, resembling sago. It is then put into an earthen pot, whose bottom is perforated with a number of small holes; and this pot being placed upon another, the two vessels are luted together, either with a paste of meal and water or with cow's dung, and placed upon the fire. In the lower vessel is commonly some animal food and water, the steam or vapour of which ascends through the perforations in the bottom of the upper vessel, and softens and prepares the *kouskous*, which is very much esteemed throughout all the countries that I visited. I am informed that the same manner of preparing flour is very generally used on the Barbary coast.'

(e) Open pedestal lamp; reddish-buff ware with brown glaze all over, except on part of foot; triangles cut into pedestal, and a large triangular hole for holding. Height 26.9 cm.

**Materials.** The clay for the body and the clay for the grey-brown slip decoration are chipped from a hillside three kilometres away, and brought in blocks to the pottery. These blocks are left, with water, in an oil drum or any hole in the floor, to soften overnight. There is no method of purifying. No broken pot is added, but sand from the floor of the pottery is mixed in with the clay during shaping (see para. 5). Galena, used for the glaze, is bought locally at the *Souk*.

**Manufacture.** (a) The wheel. A wheel exactly similar to that at Takouchtem is employed, set in a pit in the floor in the same way (Plate Ob). This seems to be fairly widespread; I saw it again farther south on the O. Draa at Tamgrout.

(b) Shaping. In general, the body is brought to the required shape by being moulded on the wheel, on top of a well fired vessel of similar shape. First, the clay mixed with water is kneaded on the floor into a flat 'pancake' shape. In this process, a good deal of the sand on the floor is deliberately incorporated. The underside of the 'pancake' is given an extra-thick coating of sand, and is laid on top of the vessel on which it is to be moulded. This vessel has itself been fastened to the wheel with wet clay.

The body is moulded by hand to the required shape, while the wheel is turned. Then several tools are brought into operation to complete the shaping of the pot:

(i) *nous*, a small bamboo slip, brought to a sharp-edged point. With this the edge of the body is cut to correspond to the edge of the mould. The moulded pot is then removed from its supporting vessel, and set up on its own base.

(ii) *talaouht*, a flat wooden or bamboo smoother (27.5 × 3.1 cm.). Used to smooth the edges of the pot.

(iii) *talomat*, a short strip of leather. This is used for the final smoothing of the edges.

(iv) *aghmed*, a small round wooden billet (12.0 × 3.2 cm.). The outside or convex surfaces are smoothed with this tool.

(v) *azrou*, a kidney-shaped stone, with which the inside or concave surfaces are smoothed.

(vi) Sometimes a strip of rubber is used to give the final polish to the inside and outside surfaces.

There is a special technique for the preparation of the near-globular vessels (see above). It can best be described by stages:

(i) The body of the vessel is prepared in the usual manner and left to harden.

(ii) For the lid, a 'pancake' of prepared clay is shaped on an old vessel lid, removed from it, inverted, and placed on top of the body of the vessel.

(iii) The *nous* is used to cut the lid to correspond to the shape of the vessel rim.

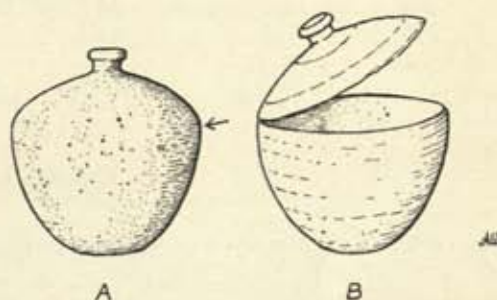


FIG. 3. SEPARATION OF LIDDED VESSELS AT TAOURIRT

(iv) The lid is turned over once more, fitted to the body of the vessel again, and then lid and body are sealed together by a thin layer of clay, the join being invisible, so that the two appear as a solid sphere (fig. 3A).

(v) In this condition the vessel is fired, taken to the *souk* and sold. The final act of the vendor is to separate lid and body with a few sharp taps of a stone, on which they fall apart (fig. 3B). Specimens were collected both sealed and broken open.

The object of this complicated technique is to ensure that when the purchaser buys his pot the lid is as near a perfect fit as possible. It may be, too, that the air pressure created inside the vessel during firing helps it to retain its spherical shape.

(c) Glazing and application of slip. Detailed observations of the glazing technique were not made. The decoration is a grey-brown slip applied in lines with a small brush of goat hairs called *khit*.

(d) Firing. A kiln similar to that at Takouchtem is used (Plate Oa), and, as at Takouchtem, there is negligence in stacking the pots, with consequent sticking together, dripping of glaze, etc. But the kiln at Taourirt is built up of mud bricks, and the times of firing are different; here, the fire is stoked for three hours approximately, and the pots left for only five hours.



It was observed that the pots, which were of a reddish ware before firing, acquired a white surface during the firing. This is a process for which I cannot account from my observations made at the time, and certainly the potters themselves offered no explanation. Unfortunately, no analyses have yet been made of the clays or the glaze.

*Conclusion.* This technique is of interest for the special method above described, in which the lid and the body of a vessel are sealed together for firing and transportation.

#### Acknowledgments

I acknowledge gratefully the advice and assistance given me by Mr. T. K. Penniman and Miss Blackwood of the Pitt Rivers Museum, and Mr. Faudree of New College, Oxford, both before and after the visit to Morocco during which these notes were made. I am also indebted to Mlle Gérard of the Musée, Kasba des Oudaias, Rabat, for the information which she gave me concerning sites of local pottery industries in Morocco. Finally, it should be said that the hospitality which I received from the Moquaddem and potters of the village of Takouchtem made the collecting of notes upon their pottery a very memorable experience.

## A PRELIMINARY INVESTIGATION OF THE BLOOD GROUPS OF THE SAB BONDSMEN OF NORTHERN SOMALILAND\*

by

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**252** While the physical characteristics of free-born noble Somali have been studied, as far as is known no attention has been given to the *sab* bondsmen.<sup>1</sup> This paper reports the results of a preliminary serological investigation of a small sample of 54 male *sab* and compares the results with those of a study of 1,000 noble Somali.<sup>2</sup> Samples of blood were obtained with the co-operation of the Department of Medical Services of the Somaliland Protectorate,<sup>3</sup> and with that of Midgaan and Tumaal elders of Hargeisa town. Our results treat these two groups as one sample.

The Midgaan (pl. *Midgo*) (hunters, leather-workers, professional barbers, etc.), Tumaal (pl. *Tumaallo*) (mainly blacksmiths), and Yibir (pl. *Yibro*) (itinerant pedlars and magicians) known to free-born Somali as *sab*<sup>4</sup> have frequently been described as 'low caste' but they are more correctly bondsmen.<sup>5</sup> The distinction is usually phrased as between *sab iyo Soomaali* (*sab* and Somali); or *Aji iyo Midgo* (noble Somali and Midgaan). Traditionally each member of this class was bound in servitude to a noble Somali family. Not every noble Somali had a *sab* bondsman attached to him but all *sab* were subject to a noble patron and protector. To a large extent this pattern persists in Northern Somaliland, although many of the *sab* have achieved a partial emancipation by moving to practise their special skills in the towns and urban centres. Out of a total Somali population of about 2,500,000 in the British Protectorate and Somalia, the total number of *sab* bondsmen is probably at the present time not more than 12,500.

\* With six tables

The Midgaan (9,000) are the most numerous, with the Tumaal (2,250) next and the Yibir (1,300), who are now rare, last.

The *sab* live scattered all over Northern Somaliland in real or putative patrilineages, on a pattern similar, though reduced, to those of their noble Somali (*Aji*) protectors. In different regions they are known locally by the names of the eponyms of the small agnatic lineage groups into which they are divided, rather than by their occupational classification as Midgaan, Tumaal and Yibir. Ultimately, however, it is as these and, collectively, as *sab*, that they are described. In the north the two largest Midgaan lineage groups are the Muuse Diiriye in the west, and the Madiban in the east. Except through their protectors, *sab* have traditionally no rights in the field of Somali political relations. Nowadays a few *sab* who have succeeded in amassing considerable wealth as traders, or who have enhanced their status by employment in government service, have achieved the right to attend and speak at the councils of the Somali lineage groups to which they are attached. But noble Somali still do not marry with them. Illicit unions sometimes occur, and a noble Somali may beget a child by an attractive *sab* woman. The Midgaan, Tumaal and Yibir marry amongst themselves but not with *Aji* Somali. Most of their activities are amongst themselves, and they have relations with noble Somali only indirectly through their patrons. They have their own sheikhs and *wadaad*<sup>6</sup> who fulfil for them the same functions as do those of their masters amongst noble Somali. All *sab* speak the dialect of the Somali to whom they are attached. But they



have also secret codes, hardly sufficiently extensive to be called languages,<sup>7</sup> which they use when they wish to conceal what they are saying from listening Somali. The *sab* themselves, and the Somali—who would not demean themselves by troubling to learn them—do not regard these dialects very seriously.

Apart from their specialist trade skills their cultural features are identical with those of Somali. The Midgaan, it is true, have songs of magical intent associated with hunting animals, but this is part of their specialist knowledge. Some of the *sab*, particularly the Yibir,<sup>8</sup> used to be called derogatively 'corpse-eaters' (*bakhti cume*) because they were alleged to eat meat considered impure by Somali. But there is little indication that this is still true today.

Various traditions are current in Somaliland of the origin of the *sab*. According to some Somali, the *sab* descend from noble Somali who became degraded by eating meat which had not been properly slaughtered in Muslim fashion during a famine. Some *sab* informants also, referring to well documented cases, maintain that they are of the same stock as the Somali but descend from small, numerically weak lineage groups which were reduced to servitude by more powerful enemies.<sup>9</sup>

In support of these claims, Midgaan informants have produced genealogies tracing descent from Dir, the founder of the Dir clan family, generally regarded as the oldest Somali stock.<sup>10</sup> Some Tumaal trace descent from Daarood, founder of the noble Daarood Somali clan family. Various writers have suggested that the *sab* represent the remnants of pre-Somali peoples conquered by Hamitic Somali invaders.<sup>11</sup> In Dr. Cerulli's view, however, whatever common characteristics the *sab* possess are to be ascribed not to common ethnic origin, but to the action of common historical processes.<sup>12</sup> Cerulli regards them as a mixed conglomerate people of partly pre-Hamitic origin.

Since there are no main cultural differences, and no strong traditions of diverse origin,<sup>13</sup> it would seem that only comparative study of their physical characteristics can throw light on their origins. Miscegenation is almost, if not entirely, forbidden, so that were the *sab* originally possessed of different physical characters to the Somali these differences should still be evident. To the casual observer most *sab* in Northern Somaliland look much the same physically as Somali. It is sometimes possible, however, to identify a Midgaan, Tumaal or Yibir. Somali indeed usually claim to be able to distinguish *sab*, but it is difficult to decide whether or not they do this only on a basis of physical features. In the presence of Somali, *sab* tend to adopt a subordinate bearing and presence. As was stated at the outset no physical study of the *sab* has yet been made as far as we know. The results of our serological investigation are set out in the following tables. They show that there is no significant difference in the distribution of the *ABO* and *MN* groups in our sample and in a sample of 1,000 noble Somali. The contingency table for the Rhesus group does not accord so exactly, but the difference is small and may be due to the size of our sample. As far as the results go, the blood-group composition of

TABLE I. THE 'ABO' GROUPS OF TUMAAL AND MIDGAAN

	OBTAINED	
	Number	Per Cent.
O	26	48.15
A <sub>1</sub>	12	22.22
A <sub>2</sub>	4	7.41
B	11	20.37
A <sub>1</sub> B	0	0.00
A <sub>2</sub> B	1	1.85
Total	54	100.00
GENES		
r	0.7050	
p <sub>1</sub>	0.1181	
p <sub>2</sub>	0.0569	
q	0.1200	
Total	1.0000	

TABLE II. CONTINGENCY TABLE TO COMPARE 'ABO' BLOOD GROUPS OF TUMAAL AND MIDGAAN ON THE ONE HAND WITH 1,000 SOMALIS ON THE OTHER

	Midgaan and Tumaal		1,000 Somalis		Total
	Obtained	Expected	Obtained	Expected	
O	26	30.74	574	569.26	600
A <sub>1</sub>	12	7.28	130	134.72	142
B	11	9.07	166	167.93	177
A <sub>1</sub> + A <sub>1</sub> B + A <sub>2</sub> B	5	6.91	130	128.09	135
Total	54	54.00	1,000	1000.00	1,054

$\chi^2 = 4.9853$ . For a value of  $\chi^2 = 4.9853$  where  $n = 3$ , the probability P lies between 0.20 and 0.10.

TABLE III. THE 'MN' GROUPS OF TUMAAL AND MIDGAAN

	OBTAINED	
	Number	Per Cent.
MM	16	29.63
MN	23	42.59
NN	15	27.78
	54	100.00
GENES		
M	0.5093	
N	0.4907	
	1.0000	

TABLE IV. CONTINGENCY TABLE TO COMPARE 'MN' BLOOD GROUPS OF TUMAAL AND MIDGAAN ON THE ONE HAND WITH 1,000 SOMALIS ON THE OTHER

	Midgaan and Tumaal		1,000 Somalis		Total
	Obtained	Expected	Obtained	Expected	
MM	16	13.68	251	253.33	267
MN	23	27.31	510	505.69	533
NN	15	13.01	239	240.98	254
Total	54	54.00	1,000	1000.00	1,054

$\chi^2 = 1.4524$ . For a value of  $\chi^2 = 1.4524$ , when  $n = 2$ , the probability P lies between 0.50 and 0.30.



TABLE V. THE RHESUS-GROUP RESULTS OF TUMAAL AND MIDGAAN

OBTAINED		
	Number	Per Cent.
CCDee	0	0.00
CcDEe	3	5.56
CcDee	20	37.03
ccDEE	0	0.00
ccDEe	3	5.56
ccDee	23	42.59
ccD <sup>ue</sup> ee	3	5.56
ccdee	2	3.70
Total	54	100.00

CHROMOSOMES		GENES	
CDe	0.2130	C	0.2130
cDE	0.0556	E	0.0556
cDe	0.4257		
cD <sup>ue</sup> e	0.1103		
cde	0.1954		
Total	1.0000		

TABLE VI. CONTINGENCY TABLE TO COMPARE RHESUS BLOOD GROUPS OF TUMAAL AND MIDGAAN ON THE ONE HAND WITH 1,000 SOMALIS ON THE OTHER

Tumaal and Midgaan			1,000 Somalis		Total	
Obtained			Expected	Obtained		Expected
CCee	}	20	14.35	260	265.65	280
Ccee						
CcEe						
ccEE	}	6	12.60	240	233.40	246
ccEe						
cc ee						
		28	27.05	500	500.95	528
Total		54	54.00	1,000	1000.00	1,054

$\chi^2 = 6.0237$ . For a value of  $\chi^2$  6.0237, when  $n = 2$ , the probability  $P$  lies between 0.05 and 0.02.

our *sab* sample is thus virtually identical with that of the Somali. While it would be dangerous to generalize from so small a sample (54 individuals), our results suggest that in serological characters there is no difference between those of the *sab* and those of noble Somali. Before a definite conclusion can be reached, however, further and larger samples of *sab* blood are required. It would be valuable to have samples from different parts of Somaliland, for it

cannot be assumed that *sab* from other parts of Somaliland have the same serological characteristics as our Hargeisa sample.

## Notes

<sup>1</sup> E.g. P. Lester, 'Étude anthropologique des populations de l'Éthiopie,' *L'Anthropologie*, Vol. XXXVIII (1928), p. 289; *id.*, 'Contribution à l'anthropologie des Somalis,' *Bull. Mém. Soc. Anthropol. de Paris*, Vol. VIII (1927), p. 175; D. R. Parenti, 'Antropologia della Somalia Meridionale,' *A.A.E.*, Vols. LXXVII-LXXIX (1947-49), pp. 89-113; N. Puccioni, *Antropologia e Etnografia delle Genti della Somalia*, Bologna, Vol. I (1931).

<sup>2</sup> A sample of 1,000 individuals was investigated by Goldsmith at Hargeisa in the British Protectorate in 1953. This study was financed by a grant from the Westminster Hospital, London.

<sup>3</sup> We are particularly indebted to the enthusiastic co-operation of Mr. H. Piper, who took most of the samples of blood. The blood was collected in 1957 while one of us (Lewis) was engaged in a sociological study of the Somali financed by the Colonial Social Science Research Council, London.

<sup>4</sup> The Midgaan, Tumaal, and Yibir (*sab*) are not to be confused with the autonomous Rahanweyn and Digil (*Sab*) tribes of Southern Somalia. Both words (*sab* and *Sab*) have, however, the same pronunciation.

<sup>5</sup> See Lewis, *Peoples of the Horn of Africa: Somali, Afar (Danakil) and Saho*, Internat. Afr. Inst., London, 1955, pp. 51-55.

<sup>6</sup> *Wadaad* is the Somali equivalent of the Arabic *Sheikh* which is also used. *Wadaad* usually denotes a man of religion whose knowledge is less than that of a *Sheikh*.

<sup>7</sup> See J. W. C. Kirk, *A Grammar of the Somali Language*, Cambridge, 1905, pp. 184-214; E. Cerulli, 'Il gergo delle genti di bassa casta della Somalia' in *Festschrift für Meinhof*, 1927.

<sup>8</sup> See Lewis, *op. cit.*, pp. 53-55.

<sup>9</sup> This is known to be the case, for example, with the *lo'jir* (literally, cattle herds) bondsmen attached to the Somali Daarood Warsangeli clan of Erigavo District of the British Protectorate. The so-called *lo'jir* are in fact people of the noble Gurgureh clan (found today in strength near Harar in Ethiopia) who were forced into servitude by a *Garaad* of the Warsangeli.

<sup>10</sup> See Lewis, *op. cit.*, p. 25.

<sup>11</sup> E. Cerulli, 'Il diritto consuetudinario della Somalia Italiana settentrionale,' *Bollettino della Società Africana d'Italia*, Naples, Vol. XXXVIII (1919), p. 32; H. Neuville, 'Notes sur l'ouabé poison,' *L'Anthropologie*, 1916, p. 375.

<sup>12</sup> Cerulli, *op. cit.* in note 7 above, p. 110.

<sup>13</sup> Some exception should however be made for the now insignificant Yibir who base their claim to collect alms on the magical conquest of their forefathers by Sheikh Aw Barkhadle, one of the founders of Somali Islam. See Lewis, 'Sufism in Somaliland: A Study in Tribal Islam—I and II' reprinted from *Bull. Sch. Orient. and Afr. Stud.*, Vol. XVII, Part 3 (1955), and Vol. XVIII, Part 1 (1956), p. 153.

## ROYAL ANTHROPOLOGICAL INSTITUTE PROCEEDINGS

**Yoruba Urbanism: A Summary.** By William Bascom, M.A., Ph.D., University of California, Berkeley. Summary of a communication to the Institute, 12 June, 1958

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Of the 5,046,799 Yoruba in Nigeria in 1952, more than half lived in communities of 5,000 and over, while more than a fifth were in cities of over 100,000 including Ibadan (459,196). Their index of urbanization, as defined by Davis and Casis, falls below that of Great Britain, Germany and the United States, but above that of Canada, France, Sweden, Greece and Poland. Estimated at 37.4 on the basis of 1931 census data,<sup>1</sup> it has been recalculated at 39.3 using the more complete and more accurate census of 1952. Within the Western Region, which includes 89

per cent. of the Yoruba in Nigeria, the index is 42.4, exceeding even that of the United States.

Six Yoruba cities (Ibadan, Lagos, Ogbomosh, Oshogbo, Ife and Iwo) have more than 100,000 inhabitants; three others (Abeokuta, Oyo and Ilesha) more than 70,000; three (Iseyin, Ede and Ilorin) more than 40,000; and twenty others have more than 20,000. Official figures for population density are lacking except for Lagos, which had reached 87,000 in 1950, with 141,000 in one of its three wards. Using the 1931 census figures and city maps of that period, approximate densities were calculated for Abeokuta at 5,720, Oyo 13,914, and Ogbomosh at 43,372 per square mile.



Urbanism is a traditional Yoruba pattern, antedating industrialization, acculturation and even European penetration. Only Lagos (267,407) follows the familiar pattern of the growth of African cities at ports, mining and trading centres, and colonial administrative headquarters. The permanency of Yoruba cities can be demonstrated over the past century by the estimates of the Census Reports of 1931, 1921, and 1911, Governor Moloney in 1890, Delany and Campbell in 1860, Bowen in 1849-56, and by Crowther's list of 'principal towns in the country of Yoruba' in 1841.

Yoruba territory was first explored in 1825 and 1830 by Clapperton and the Lander brothers, who passed west and north of the major cities of today. Old Oyo was the largest Yoruba city that they visited, and one may infer that Old Oyo, Ilorin (40,994), Igboho (8,476), Kishi (7,827) and 'Kooso' (unidentified) were then 20,000 or over. Four others were probably above 10,000, including Shaki (22,983), Irawo (2,893), 'Assoudo' and 'Duffo,' and in former times 'Essalay.' Seventeen others were or had recently been over 5,000, of which Ilaro (12,400), Igbeji (582), Ipokia (3,607), Ihunbo (774) and Ijana can be identified.

Many of these were obliterated during the course of the last century when the Yoruba were torn by wars with Ilorin on the north and Dahomey on the west, and internally by those between rival Yoruba states and cities. Old Oyo was abandoned about 1839, and to cite only one other example Ijaye (1,478), which had been estimated variously from 35,000 to 80,000, was 'broken' in 1862. New Oyo, Ilorin, Ibadan, Abeokuta and Lagos became urban centres only during the last century, but Ketu (10,028) is referred to in Dalzel's account (1793) of its defeat by Dahomey in 1789, and others had been known from still earlier periods.

Old Oyo was known a century before Clapperton and Lander's visit. It is mentioned in d'Anville's map of 1729, Labat's account (1731) of a visit in 1725, Snelgrave's report (1734) of Dahomey's conquest of Ardra in 1724, and Barbot's comments (1732) on the invasion of Ardra in 1698. It is possibly referred to as Ulkami by Dapper (1668) and later writers. During the eighteenth century Oyo repeatedly interfered in the affairs of Dahomey and its neighbours and collected an annual tribute until about 1827, when it was embroiled in the wars with Ilorin which led to its downfall. It is not unlikely that the first Portuguese explorers to reach Benin City in 1486 brought back reports of Ife (110,790), which spurred on their exploration of the sea route to India in the belief that its king was Prester John.

Ijebu-Ode (27,558) appears in the literature over the past 450 years. Pacheco Pereira (1507-8) mentions it as 'a very large city called Geebuu,' and it is shown on a Portuguese map (c. 1500) as Ciudad de Jabu. Thereafter it is mentioned by D.R. (1602), Alonso de Sandoval (1647), Dapper (1668), de la Croix (1688), a Portuguese map (1700), Barbot (1732), d'Anville's maps (1729 and 1743), Norris (1789), Dalzel (1793), Adams (1821 and 1823), Clapperton (1829), Crowther (1843) and d'Avezac (1845), and in 1854 it was visited by Hinderer and Irving.

These communities therefore satisfy three of Wirth's criteria of a city: size, density and permanency. His fourth criterion, heterogeneity, is not clearly defined. Both Miner in his study of Timbuctoo (6,000) and Schwab in his study of Oshogbo (122,728) had

difficulty in deciding whether they were dealing with a city or a folk community. The distinction between folk and urban and Wirth's definition of urbanism require re-examination.

Yoruba cities were non-industrial, being based on farming, craft specialization, and trade involving large markets, true money and true middlemen. Farming is not only a rural occupation; city dwellers work outlying farms which surround the city. The production of many other goods and services is specialized, with technological skills and ritual knowledge restricted to small groups of specialists. As a result the city is an economic unit, within which each individual is dependent on his neighbours for goods and services which he cannot provide for himself. This degree of specialization and economic interdependence may be a more useful criterion than heterogeneity.

Yoruba cities were heterogeneous only in terms of craft specialization, social stratification, and social and political segmentation. They were non-cosmopolitan, lacking the ethnic heterogeneity which characterizes Timbuctoo. Some, like Oyo, Ife and Ijebu-Ode were metropolitan. As capitals they served as centres of kingdoms of varying size, collecting taxes and maintaining political and military control over villages, towns and other cities which acknowledged their sovereignty. As Wirth suggests for cities in general, a distinctive mode of urban life is evident; and market transactions are characterized by contacts which are 'impersonal, superficial, transitory, and segmental.' In this sense Yoruba cities were clearly secondary groups, while the lineages of which they were composed were primary.

On the other hand there is no evidence that city life tended to weaken kinship bonds or produce the increased mobility, instability and insecurity which Wirth suggests are the results of heterogeneity. Anomie is not apparent, except among rural Yoruba who find themselves in the city. The lineage is basic to Yoruba society, urban as well as rural, and it is in large part through the lineage structure that 'interests are made effective through representation.'

The bonds of kinship and living together which unite the lineage were strong, not absent; and the elements of competition and formal control mechanisms were developed, not as substitutes for kinship controls as Wirth suggests, but rather as mechanisms of control on a secondary, supra-kinship level. Though Wirth specifically dismisses political organization as an unsatisfactory criterion of urbanism, the presence or absence of a formalized city government which transcends primary groups such as lineages and incorporates them into a community would seem more useful than heterogeneity for cross-cultural comparisons, when coupled with size, density and permanency.

#### Note

<sup>1</sup> 'Urbanization Among the Yoruba,' *Amer. J. Sociology*, Vol. LX, No. 5, March, 1955, pp. 446-54. For further discussion, details and bibliography, see this and 'The Historical Evidence of Yoruba Urbanism,' *Présence Africaine* (forthcoming); 'Urbanism as a Traditional African Pattern,' *Sociol. Review* (forthcoming); 'The Fall of Old Oyo or Katunga,' *Présence Africaine* (forthcoming); and 'Lander's Routes Through Yoruba Country,' *Nigerian Field* (forthcoming).

## SHORTER NOTE

**A Note on Graphical Osteometry and Evolution.** By Professor L. S. Palmer, D.Sc., Ph.D., Hon. Curator of Wells Museum, Somerset. With a text figure

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Osteometric data are dimensional measurements concerned with the size of bones; for example, the volume of a skull, the area of a palate, the angle in the symphyseal region of a

mandible or the length of a long bone. Ordinary anatomical indices, being the percentage ratio of any two osteometric measurements, determine the variation of one measurement with respect to the other, and the resulting index may or may not be dimensional. It is sometimes desirable to consider both absolute measurements and indices as variables, the second parameter being



either space or time. This note is not concerned with spatial or geographical variations but with temporal variations. Furthermore temporal variations concerned with the growth of an individual and their graphical representation are already familiar and will not therefore be considered here. But morphological changes associated with the antiquity of the human skeleton are much less familiar, and it has not been possible until quite recently to depict such changes in graphical form. It is because of the many advantages to be derived from a graphical treatment that it is felt desirable to outline in this note a method of producing comparable graphs from both absolute osteometric measurements and relative anatomical indices whether they are dimensional or non-dimensional.

The inability to do this in the past arose first from the scarcity of human skeletal material from which data for graphical ordinates could be determined, and secondly because the relatively new science of geochronology has only recently been able to date prehistoric bones with sufficient accuracy to serve as abscissae. The first limitation has been overcome by the fairly recent discoveries of human remains in Italy, Palestine, Java, China and Africa which supplement the earlier discoveries in Europe; whilst the second difficulty has been met by improved methods in the dating of geological deposits, by a greater knowledge of the typology of associated artifacts, by pollen analyses and by such physical techniques as those based upon measurements of carbon 14 and fluorine.

Assuming the availability of a number of similar bones from different geological horizons, the problem reduces itself to plotting the particular anatomical measurements or some function of the measurements as ordinates against the date or antiquity of the particular skeletons as abscissae. This is not quite so simple a problem as it might first appear. To take full advantage of a number of graphs each showing the temporal variation of some particular measurement or index, it is desirable that they should be comparable. Some measurements increase with the antiquity of the skeleton or bone and may even change sign, such as the height of the nuchal crest above the Frankfurt plane; others, for example the volume of the skull, may increase during one period and decrease during another; whilst the medial width of the sphenoid bone, for instance, steadily decreases with antiquity. Again the magnitudes of the ranges of variation and the differences in dimensions present difficulties when it becomes necessary to compare the morphological changes of different parts of the skeleton.

It is not proposed in this note to show how all these and other difficulties may be overcome by converting all measurements and indices to non-dimensional percentages of the average values for the modern male adult European; the indices for which are all made equal to 100. When this is done (see the Appendix), the values of the modified indices for the bones of the more primitive skeletons measure the 'percentage humanity' of the particular bone. On plotting the modified measurements against the time or date of the skeleton, the resulting graphs for each index approach 100 for the average man of the present day and their earlier temporal variations are directly comparable one with another. In order to appreciate this procedure two examples are shown in the graphs of fig. 1.

Before considering these particular cases, some of the advantages of a graphical treatment of variable parameters will be mentioned briefly. This seems to be desirable because Dr. E. H. Ashton when criticizing (in *MAN*, 1958, 189) the use of the graphs depicted in *Man's Journey through Time* showed such confusion of thought concerning the conclusions deduced from the graphs that his criticisms were quite meaningless to anyone familiar with the application of the principles of scientific method to graphical

interpretations. As is well known the essence of scientific method is an alternating sequence of experimental measurements and theoretical deductions; the probability of the correctness of the latter being dependent on the accuracy and number of the former. The original theory suggests further experiments which lead to a modified or supplemented theory, and so on. In a similar way the number and accuracy of the plotted points on a graph determine the degree of probability of the truth of the theoretical interpretations. Thus any reference to genetic relationships between the individuals whose skeletons provide the osteometric data of a physical-development/time graph is purely a theory with a degree of probability, in this case, not only depending on the accuracy and number of the measurements but also on their linearity and on the geographical or spatial distribution of the skeletal remains. Sir Wilfred Le Gros Clark emphasizes that an important criterion of a genetic relationship is a temporal sequence of morphological changes. Without some continuous sequence there can be no question of a genetic relationship, but with a continuity a genetic connexion is possible, provided that other factors upon which genetic relationships depend are also present. Similarly, deductions based on extrapolations are theoretical with a probable reliability depending on the regularity of the form of the graph and on the temporal length of the extrapolation. Extrapolations can yield valuable information as long as they are used with a full appreciation of their limitations. It was in these interrelationships between graphical data and theoretical interpretations and extrapolations that Dr. Ashton in his article referred to above showed such complete disregard for the elementary principles underlying a graphical treatment of skeletal data.

Examples that may be used to illustrate these principles are at present limited to skulls and possibly long bones (especially femora) which alone have been preserved in sufficient quantities to provide adequate numbers of points on development graphs. Furthermore, selected remains need to be distributed as uniformly as possible throughout the million or so years of man's existence on this earth. The measurements chosen should preferably have taxonomic significance in case the graphs justify the assumption of at least some genetic relationship between the several skeletons. In such circumstances any linear graph, when translated into an algebraic equation, would represent a law of evolution of that morphological feature with which the measurements are specifically concerned.

In conformity with these requirements the cranial capacity and the angle in the symphyseal region of the mandible have been selected for illustrating this graphical method of recording the temporal variations of osteometric data. The measurements of cranial capacity and of the angles in the symphyseal region of the mandible (or of the lower dental arcade) together with the respective dates of the skeletons are those which have been recorded in *Man's Journey through Time*, 1957, Tables II and VIII. Graphs I<sub>1</sub> and I<sub>2</sub> in fig. 1 are plotted from these values, and the 20 or 30 skeletons are recorded in chronological order along the top of the figure. In order to relate the dates with the glacial periods, the durations of the latter are indicated below the abscissa axis.

It will not be feasible in this note to give a full interpretation of these graphs. Consequently only a few of the possible theoretical deductions will be considered.

1. Assuming some genetic relationship between the Pithecanthropoids, man's rate of evolution as determined from the gradient of the earlier parts of the graphs is a little over one darwin. This value for the rate of evolution accords closely with the value calculated by Dr. F. Weidenreich (namely 1.03 darwins) and with the value of 1.28 darwins deduced from the application of Haldane's exponential law of human evolution to cranial capacities.



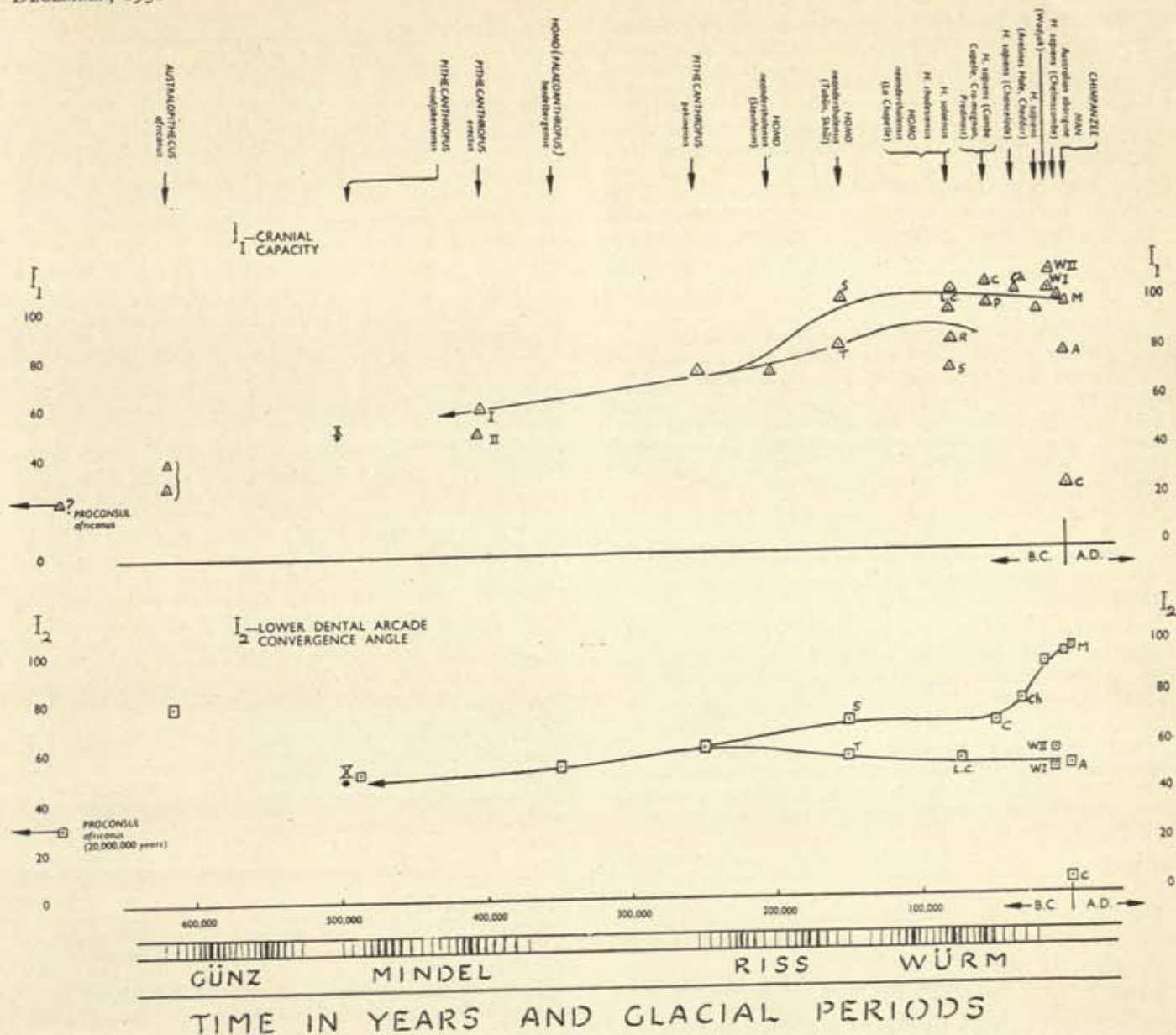


FIG. 1

2. The bifurcation of the graphs about 250,000 years ago may indicate a case of divergent evolution when the Neanderthaloids began to specialize. Such a probability has already been suggested by Sir Arthur Keith when discussing the Mount Carmel remains. On the other hand, if the data for the Swanscombe skull be included, the upper branch of curve I<sub>1</sub> would tend to continue backwards in time more or less parallel to the lower branch. In this case it might be concluded that the Swanscombe man, the pedomorphic Neanderthaloids and *Homo sapiens* developed independently of the Pithecanthropoids and the gerontomorphic or specialized Neanderthal men. But any such theoretical interpretation, based as it is on the very approximate measurements of one skull together with an equally approximate date for the geological horizon of the relics, means that this particular view of human evolution has a very low degree of probability until more skeletal remains are discovered.

3. The mandibular index of the Australopithecinae falls considerably above the  $I_2$  graph. This is also the case for other measurements but not for the cranial capacity. Consequently it is a reasonably reliable conclusion that these men-apes are not ancestral to man. To claim any genetic relationship would involve an inexplicable, if not an impossible, jump in the rate of change of these particular mor-

phological characteristics. This interpretation agrees with that previously deduced by Professor Sir Wilfred Le Gros Clark from a study of the milk molars of these creatures.

4. In view of the unexpected linearity of the graphs, it is theoretically sound to extrapolate them. But as has been explained above, the degree of reliability of the consequent theoretical deductions must be taken into consideration.

When extrapolated backwards the graphs pass moderately close to the values for *Proconsul africanus*. This does not mean that man has descended from this Miocene ape but it does mean that he could have done. This suggestion has already been made by Sir Wilfred Le Gros Clark and the present agreement indicates the value of making theoretical deductions of this kind from a graphical presentation of skeletal measurements.

Extrapolations forwards of the upper branches suggest little change in man's cranial capacity but a possible increase in his speech potentiality during the next few decades. But forward extrapolations of the lower branches pass close to the corresponding values for the Australian aborigine. It is therefore tempting to conclude that the gerontomorphic Neanderthal man may be ancestral to the Australian native. The Wadjak jaws (but not their skull capacities) give



strong support to this suggestion. It is also in accord with the work of Dr. E. K. Tratman, who has detected Neanderthal characteristics in Mongolian teeth. But, as Dr. A. M. Morant has pointed out (*Annals of Eugenics* II, 1927), there is no indication of any genetic relationship from facial characters.

These few theoretical interpretations from experimental data displayed in graphical form will, it is hoped, serve to illustrate the value, and also the limitations, of a graphical treatment of the temporal changes in osteometric measurements. Some reference will be made to interrelations between changes in human morphology and cultural development in a second note to appear in a future issue of MAN.

## APPENDIX

*The Transformation of Normal Measurements and Indices to 'Percentage Humanity' Values*

Let  $I$  be the normal value and  $H$  the transformed value of a given measurement or index, and let  $I_P$  represent the value of  $I$  for the earliest and most primitive member of the series and  $I_M$  the corresponding value for the average modern adult male European. Also let  $I_X$  be the value of the index for any intermediate skeleton. To transform values of  $I$  to the corresponding 'percentage humanity' values  $H$ , it will be necessary to consider two cases:

(i) when  $I_P < I_M$  and (ii) when  $I_P > I_M$ .

The problem is to make  $I_M = 100$  and to transform all the other indices to numbers less than 100 without altering their original relative values.

(i)  $I_P < I_M$

The first case is relatively simple for it entails adjusting  $I_M$  to 100 and changing the other values of  $I$  in proportion. Thus the modified value  $H_X$  of any index  $I_X$  is given by:

$$H_X = 100 I_X / I_M \quad \text{Equation (1)}$$

For example, the average value of the angle in the symphyseal region of the mandible for modern man is about  $48^\circ$ . For the classical Neanderthals it is of the order of  $26^\circ$ . Hence the 'percentage humanity' of these two mandibles is given by:

$$H_M = 100 \times 48 / 48 = 100 \text{ per cent.}$$

$$\text{and } H_X (\text{Neanderthal}) = 100 \times 26 / 48 = 54 \text{ per cent.}$$

(ii)  $I_P > I_M$

The transformation in this case is a little more complicated because the series of index values increase with the antiquity of the skeleton. The procedure is to make an intermediate transformation which reverses the gradient of the ( $I$ /Time) graph, thus making the indices decrease with antiquity. Let the intermediate value of any index  $I_X$  be  $Y_X$ . Then:

$$Y_X = (I_P + I_M - I_X)$$

It is now a straightforward operation to substitute these intermediate or  $Y$  values into equation (1) in order to give the 'percentage humanity' values  $H$ .

Thus  $H_X = 100 Y_X / Y_M = 100 (I_P + I_M - I_X) / I_P$  Equation (2)

For example, the Stephanion/Auricular Height index measures the percentage ratio of the height of the Stephanion point above the Frankfurt plane to the auricular height of the skull. Values of this index increase with antiquity. For the two *Pithecanthropus erectus* skulls the value is roughly about 95. For the Chapelle aux Saints Neanderthal skull it is 77 and about 58 for modern man.

Hence the 'percentage humanity' values for these three types of skull are given by:

$$I_P (P. erectus) = \frac{100(95 + 58 - 95)}{95} = 61 \text{ per cent.}$$

$$I_X (H. neanderthalensis) = \frac{100(95 + 58 - 77)}{95} = 80 \text{ per cent.}$$

$$\text{and } I_M (H. sapiens) = \frac{100(95 + 58 - 58)}{95} = 100 \text{ per cent.}$$

Finally, it should be noted that when the values of any members of a series are negative, all the values should be made positive before applying either equation (1) or equation (2) to transform the series. This is easily done by adding the largest negative value to every member of the series.

## CORRESPONDENCE

**Iron Gongs from Northern Rhodesia.** Cf. MAN, 1955, 30. With two text figures

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SIR.—Mr. James Walton in his article on 'Iron Gongs from the Congo and Southern Rhodesia' made occasional reference to gongs found in Northern Rhodesia, and this has prompted the following note.<sup>1</sup>

The distribution of iron gongs in Northern Rhodesia appears to be restricted to those tribes which have, or are believed to have, entered the territory from the north or north-west (see fig. 1).

All the specimens mentioned below have welded flange joints; similarly all double gongs mentioned are two-toned.

Single gongs have been collected from the Lunda, Luchaze, Swaka and Soli peoples. In some cases the handle has been bent to form a loop; in others it is straight. The former were perhaps suspended by means of a string, the latter probably held in the hand.<sup>2</sup>

Bifurcating double gongs have been found among the Lozi, Bisa and Luchaze tribes. The Lozi and Bisa specimens are especially similar both in the shape of the gong mouths and in the spiral working on the shafts (fig. 2a).

Holub's bifurcating double gong<sup>3</sup> seems merely to be a very free interpretation of the normal Lozi type, though it lacks the spiral working on the shafts.

There appear to be two distinct groups of double gongs of the arched-connecting-link type, according to the shape of the link. The first,<sup>4</sup> an example of which has been obtained from Cazembe of the Lunda (and also, Mr. Walton tells us, from the Kwango, Urangi and Zimbabwe), has a simply arched link. The second, to be found among the Nkoya, Soli and Lala, has a link which has been

bent 'as though in manufacture it has been shaped round a round piece of wood'<sup>6</sup> (fig. 2b).

The only example of a striker in the Museum collections is made of wood and tipped with rubber. This is the normal type of striker

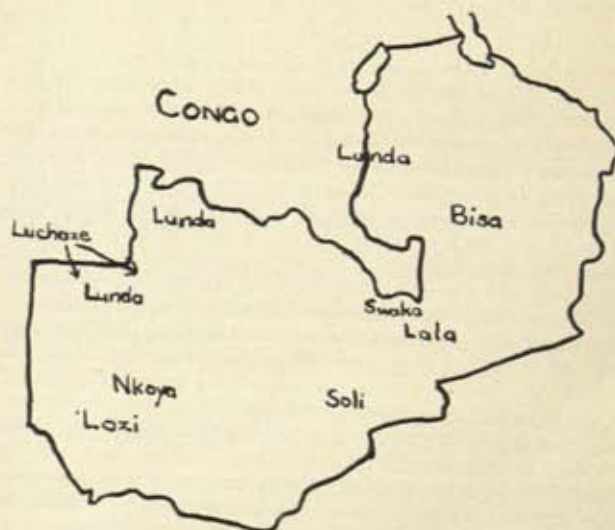


FIG. 1. SKETCH MAP OF NORTHERN RHODESIA



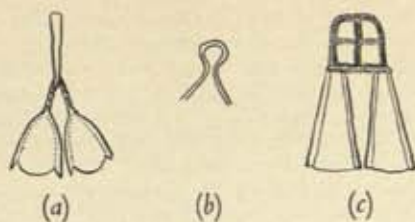


FIG. 2

or hammer used in the territory with xylophones or wooden slit drums.

These gongs are associated with the office of chieftainship; indeed, among the Lala 'any commoner who had one in his possession was liable to death'.<sup>8</sup> They are beaten only on certain important occasions: to herald the approach of the chief<sup>7</sup>; on the occasion of his death<sup>8</sup>; to summon the people in time of war<sup>9</sup>; after the killing of a lion or a witch<sup>6</sup>; and to make a cheerful noise when a ruling chief is travelling.<sup>6</sup>

In discussing the manner of suspension of the double-ended single gong<sup>9</sup> from Zimbabwe, a comparison is drawn by Mr. Walton with 'a gong, *kinguvu*, from the Kasai district which is used for sending messages and communicating alarms.' This is rather an unfortunate comparison in that the term 'gong' has two different meanings: (a) a clapperless bell, usually of metal (the Zimbabwe gong?); (b) a wooden slit drum, used for signalling purposes (the Kasai gong). Whether the Zimbabwe gong was used 'in a similar fashion' I do not know, but it is worth remembering that there is more than one way of suspending a slit drum.

Mr. Walton says that: 'The distribution of the sistrum type of double gong follows the well defined migration route along the southern margin of the Sahara.'<sup>10</sup> His distribution map,<sup>11</sup> however, shows three such gongs west of the Niger delta, one in Egypt, east of the Nile, and one in the Sudan, i.e. almost at the two ends of the migration route. Where, also, might the information on these five gongs be found?

Is the following statement based on Wainwright's thesis<sup>12</sup> (subsequently attacked, I would consider very successfully, by Schofield,<sup>13</sup> Huntingford<sup>14</sup> and Jensen<sup>15</sup>)?—'. . . the foundation of the Monomotapa Empire by Hima invaders at the end of the fourteenth century.'

Dr. Cunison<sup>16</sup> has criticized, more competently than I can, the suggestion that 'Kazembe's people took the gongs to Zimbabwe.'

I would, however, like to add that Hall and Neal fail to state where, in Diogo de Couto's works, they found the statement<sup>17</sup> which they attribute to that writer and that an examination of those portions of these works contained in Theal's *Records*<sup>18</sup> has not brought such a statement to light.

Mr. Walton's classification of gongs I have found to be very useful in dealing with Northern Rhodesia. Where in his classification, though, would one place the double gong from the Cameroons? This specimen has welded flange joints and is two-toned. It consists of two completely separate, single gongs joined together by a non-metal substance, in this case probably palm string (fig. 2c). A similar gong appears in Dr. Buschan's work.<sup>19</sup> BARRIE REYNOLDS  
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## Notes

<sup>1</sup> MAN, 1955, 30.

<sup>2</sup> This article is based on the collection of gongs at the Rhodes-Livingstone Museum.

<sup>3</sup> Pigafetta in his description of the 'Congo' and 'Angola' armies provides an interesting note on the manner in which these gongs are used: 'Another sound is made by an instrument in the shape of a pyramid, but turned upside down, as they are pointed at the bottom and wide at the top. This instrument has thin plates of iron, hollow inside, and is like a bell turned upside down. They are struck with rods of wood.' (*History of the Kingdom of Congo*, translation by Margaret Hutchinson, 1881, p. 35.)

<sup>4</sup> E. Holub, *Sieben Jahre in Sud Afrika*, 1881, Vol. II, p. 193.

<sup>5</sup> There are also very strongly defined midribs on this specimen.

<sup>6</sup> J. T. Munday, Museum files, 1943.

<sup>7</sup> Museum files, 1935.

<sup>8</sup> *Ibid.*, 1936.

<sup>9</sup>, <sup>10</sup>, <sup>11</sup> Walton, *ibid.*, fig. 1, 3 and page 22.

<sup>12</sup> G. A. Wainwright, 'The Founders of the Zimbabwe Civilization,' MAN, 1949, 80.

<sup>13</sup> J. F. Schofield, MAN, 1951, 280.

<sup>14</sup> G. W. B. Huntingford, MAN, 1952, 118.

<sup>15</sup> A. E. Jensen, MAN, 1952, 150.

<sup>16</sup> Ian Cunison, MAN, 1955, 157.

<sup>17</sup> R. N. Hall and W. G. Neal, *The Ancient Ruins of Rhodesia*, 2nd edition, 1904, p. 134.

<sup>18</sup> G. M. Theal, *Records of South-Eastern Africa*, 1950, Vols. II and VI.

<sup>19</sup> Dr. G. Buschan, *Illustrierte Völkerkunde*, 1922, Plate XIV, No. 9.

## REVIEWS

## ASIA

**Archaeological Researches in the Edsen-gol Region, Inner Mongolia, Part I.** By Bo Sommarström. *Reports from the Scientific Expedition to the North-Western Provinces of China under the Leadership of Dr. Sven Hedin*, Publ. 39. Stockholm (Statens Etnografiska Museum), 1956. Pp. viii, 188, 10 half-tone plates, 40 collotype plates, 83 illus., 3 maps. Price Sw. cr. 105 and 117.

Much of the archaeological material collected by Folke Bergman in Inner Mongolia and East Turkestan between 1927 and 1934 remained unpublished at the time of his death in 1946, though he had prepared a complete catalogue of it. Some of the material relating to the Edsen-gol region has now been prepared with painstaking exactitude and published by Dr. Sommarström, and a second volume to complete the survey is promised. The present work gives short chapters on the geography, historical cartography, prehistory and history of the region, followed by an account in minute detail of the sites examined and finds made in three areas. These are the Ikhen-gol delta (the most northerly part of the region), the western side of the Ikhen-gol and along the Ontsein-gol (two roughly parallel lines of ancient defences were investigated) west of Khara-khoto, and the Khara-khoto area or Etsina oasis, east of the Ikhen-

gol (but excluding Khara-khoto itself). There follow ten plates of photographs of the area and 40 plates illustrating some 700 archaeological finds. The maps show the Edsengol region in its setting, the region as a whole, and finally, in more detail, the Khara-khoto area. Dr. Sommarström's work is a model of scholarly care and clarity.

Some finds relating to the Stone Age in Mongolia were made, but the majority date from historical times. The Edsen-gol region formed part of China during the Han dynasties, and frontier fortifications were erected from that time onwards. It was subsequently occupied by Tibetans and by Turkish tribes and in 1036 fell into the hands of the Tanguts and formed part of the Hsi-hsia empire. In 1226 the city of Khara-khoto was occupied by the Mongols and in 1372 it was recaptured by the Ming and probably then destroyed. Little is known of the region until it was assigned to the Torguts in 1729.

Sites of several sorts were investigated, watch towers, houses, temples, stupas, and so on. Only two graves were found in the entire region; a fact explained by Dr. Sommarström as being due partly to there being no identification marks above ground, so that the discovery of graves is only fortuitous, partly to the Chinese practice of repatriating the bodies of their dead, but probably in the main to this 'area of Chinese settlement' having been inhabited



much of the time by non-Chinese tribes who did not bury their dead. Amongst the multifarious objects discovered either on the surface or by digging (sometimes unsystematic, p. 46) were many coins, dating mostly from the eleventh century, but some later and some from as early as Han times. The quantity of objects discovered and the variety of categories—household articles, ornaments, coins, Buddhist cult objects and so on—bear witness to the existence of settled cultures of considerable duration. As is to be expected, the nomads who have at times moved over the area have left far scantier traces.

C. R. BAWDEN

### The Symbol of the Beast: The Animal-Style Art of Eurasia.

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By Dagny Carter. New York (Ronald Press), 1957. Pp. ix, 204, 48 plates. Price \$8.50

Mrs. Carter was a traveller and collector before she entered the field of Animal-style art as a scholar. This is an advantage in writing a book for readers who may be coming new to the subject with a need to digest what previous scholars have done and said. Moreover, she covers, quite literally, so much ground that no considerations of possible relevance have been left out, remote though many of them seem from the area usually accepted as within the scope of such terms as 'Animal Style' or 'Scythian Beast-style,' namely, the sweep of Southern U.S.S.R. from, say, the Crimea to the wall of China. This style has also been called 'nomadic,' but this term again is anthropological, referring to some community of culture patterns but pinning down nothing artistic as regards the degree of native originality or the provenance of possible later influences, as nomadic cultures are spread through the whole area, differing ones at that.

So Mrs. Carter is right to start from the pieces themselves and settle the terms of reference as she goes along. In this way the reader proceeds from the focal points of South Russia and the Altai by lively jumps to the Ordos, back to Luristan and then west to Europe, following pieces, described or photographed, all the way. Some few of the jumps do seem a little far-fetched (from Viking and Vendel to Chou China, for instance) and syllogistic in reasoning: animal style always has wriggly animals; these both have wriggly animals, therefore they are of the same animal style. These are but minor detractions, or not even that; only distractions of a speculative kind. But more on the vitally important Pazyryk group, for example, about acupuncture (from China?), reindeer masks (from the Tungus?), horse-breeding stocks and harness patterns (from Persia?) and the mixture of skull types found in the tombs would have been more to the point than much of the later chapters on possible European influences carried over from the steppes by largely hypothetical itinerant metal-workers.

The photographs are good but not so plentiful as one could wish. Admittedly, there are never very many new plates available on this subject, but pictures of objects for comparison (details of Chinese bronzes, Hittite or Sumerian monsters, Middle Eastern carpets and so forth) would have been handy. Blocks in the text would have brought out similar features more excitingly and much more convincingly. The photographs of Mrs. Carter's own Ordos bronzes and her comments on the derivation of this style from a bone-carving culture are interesting.

In the main, though, the writer has selected and quoted in a most judicious way from such great all-rounders as Karlgren, Minns and Rostovtzeff and from quite recent publications on the important Altai sites by Kiselev and Rudenko. The bibliography, thus, is long, but Mrs. Carter often mentions in the text which are the recent and authoritative books for general coverage and this is helpful advice in approaching a subject so specialized and yet so diffuse as this.

PETER J. BEE

**Die Negrito Asiens, Vol. II.** By Paul Schebesta. Wien-Mödling (St. Gabriel-Verlag), Part I: 1954. Pp. 340. Part II: 1957. Pp. 336. Price 230 and 220 Austrian schillings

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A standard work on the African Pygmies is the volumes by Paul Schebesta which deal one after the other with the history, geography, surroundings, demography and anthropology of the Ituri-Bambuti, their ethnography, their economics, their social life and, lastly, their religion. A shortened account of this

work was published in 1952 by the Académie Royale Coloniale Belge, under the title 'Les Pygmées du Congo Belge.'

This work was followed by a series of publications on the Pygmies of Asia, of which the first volume—dealing with the history, geography, surroundings, and demography of the Negrito—appeared in 1952 as Vol. VI of the *Studia Instituti Anthropos*.

We have now before us in two parts the monumental second volume of Schebesta's study of the Asian Negritos. These two parts form Vols. XII and XIII of the *Studia*. They are devoted to the ethnography of the Negrito and deal mostly with the Semang, but much information can be gained on the Aeta, the Andamanese and also the Senoi. The Senoi are, of course, racially and culturally different from the Negrito, but their influence on the Semang is so great that it is not possible to discuss the culture of the Semang without discussing that of the Senoi. The part on the economics of the Negrito describes their housing (caves, temporary arrangements to protect against the wind, tree huts and 'pile dwellings'), clothes, ornaments, food and the methods of gaining it such as the designing of traps and the manufacture of nets and slings, bows and arrows together with arrow poisons, blowpipes and the role of the hunting dog, fishing and gathering of honey. Other activities are metalwork, the use of boats and rafts, trade, and there are some interesting observations on children's toys. The sociology of the Negrito deals with the role of the family, private property, marriage, divorce and adultery. There are some valuable observations on diseases and it is noteworthy that whereas the author did not see any syphilis in 1924-25 he noted this infection in 1939.

The second half-volume deals mostly with the religion of the Semang and with the concept of the *Orang hidup* which is Malayan for 'the immortals.' Observations on mythology and shamanism lead on to a description of the art of the Semang and of their dances and music. It is here that the influence of the Senoi is particularly noticeable. About 50 pages are given to the religion of the Aeta and there is also a brief discussion of the religion of the Andamanese. All the Negritos tend to place the Highest Being in heaven and connect it with thunder and storm. There is a tendency to have a dual god, one for the thunder and one for the echo, or in the case of the North Andamanese one for each of the two monsoons. Symbols of this dualism are double mountains and double rocks. The greatest crime amongst the Negritos is incest and there are severe sanctions against watching the copulation of animals as well as, in the case of the Semang and the Aeta, against sexual intercourse during the day and in the camp (I noted open sexual intercourse amongst the Onge of the Southern Andamans during the day and in their camp). Most of the Aeta know of animal sacrifices and occasionally plant offerings are brought by the Semang. None of these are known to the Andamanese.

Both volumes bring an exhaustive collection of the literature and numerous photographs, not all of them equal in standard. It would be presumptuous to offer judgement on this great contribution. One would have to be a Schebesta to be able to criticize or praise with sufficient authority. All one can say is that we are grateful to the author for having given us this rich treasure trove of material on the Negritos of Asia.

HERMANN LEHMANN

**Le Théâtre d'Ombres à Kelantan.** By Jeanne Cuisinier. Paris (Gallimard), 1957. Pp. 251, 16 plates. Price 1,250 francs

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Kelantan linguistically belongs to the Malay Peninsula and Indonesia; geographically it adjoins Thailand of which it was a dependant for considerable periods of its history. This border country, not favoured by prosperity, the patronage of a wealthy court or a seat of learning, is the meeting place of the Thai and the Javano-Balinese shadow theatres. Mlle. Cuisinier confines her book to the last-mentioned, but eight out of her 31 illustrations demonstrate clearly the difference between the shadow puppets of Thailand and Indonesia.

In Kelantan, in the nineteen-thirties, the religious character of the shadow play was still very much in evidence and Mlle. Cuisinier devotes 66 pages to a careful description of the ritual. In 40 pages she gives the Malay text of the performance, accompanied by a literal French translation on the right-hand page.

Malay and Dutch are among her linguistic attainments and her



conversance with the extensive literature concerning *wayang* (shadow play) is shown by her bibliography of 120 items of reference. She is, with reservations, an adherent of the theories of Rassers, who pioneered in this field with his work on Panji (who is also the hero of part of the Kelantan shadow play). She has had the perception to recognize in Kala the demoniacal aspect of Siwa, the presiding evil genius (book cover), and draws attention to Bali, where the shadow play has hardly been studied. There the Kama aspect of Siwa is as strong as is that of Kala—a subject on which I hope to write a book.

Mlle. Cuisinier's footnotes are not always convincing and much in the text still needs explanation. A certain carelessness is apparent in the bibliography, but these blemishes do not diminish our gratitude to the author for this well balanced and well written book with its valuable new material.

C. HOOYKAAS

**Nous Avons Mangé la Forêt de la Pierre-Génie Gôo:**  
**Chronique de Sar Luk, village Mnong Gar.**

**260** By Georges Condominas. Paris (Mercure de France), 1957. Pp. 495

This book is a study by a very competent French anthropologist of a primitive village in the mountainous region near Dalat lying north-east of Saigon in Viet Nam. Until now our information concerning this extremely interesting ethnographical region has been based on a few very inadequate journal articles, most of which have appeared in highly inaccessible French periodicals published in the Far East; Condominas's book is thus extremely welcome. However, the professional anthropological reader needs to be warned that the author has set out to please a wide public. Although this is a valuable book, it is not an academic monograph.

The Mnong Gar are a matrilineal people with a very interesting social organization. The author tells us that he has written a study of the kinship system but this still awaits publication; in the present volume, the basic kinship structure is somewhat glossed over. The book has the form of a chronicle of events in a single village covering a period of about 15 months. Each chapter centres on a particular episode, characteristic of village life but flavoured with the exotic. For example, Chapter II describes the prestige feasting of a village elder with the complex set of quarrels and marriage negotiations which accompanied it. Chapter III is concerned with a case of incest and suicide, Chapter IV with ritual visits to the Underworld and so on. Individuals are specified by name, and we come to know them quite well in the course of the story. The style throughout is vivid but for the most part the author has been content to write simply as an observer. Analytical comment is very limited. To help the European reader who may get tangled in the wealth of proper names and complex Mnong Gar verbal categories, the author has provided a series of lavish indexes and cross-referenced glossaries. The indexes also include an important list of botanical terms.

It cannot be claimed that the book provides us with a comprehensive understanding either of the social structure or of the ethnography of this very interesting society, but it is enormously superior to anything which we have had before from this part of the map, and it deserves a most hearty welcome. May we hope that the book sells so well that the author can publish his more professional analysis with the minimum delay?

E. R. LEACH

**Cambodia: Its People, its Society, its Culture.** By David J. Steinberg with Chester A. Bain, Lloyd Burlingham, Russell G. Duff, Bernard B. Fall, Ralph Greenhouse, Lucy Kramer, Robert S. McLellan. New Haven, Conn. (H.R.A.F. Press), 1957. Pp. 345. Price \$7.75

**261** Seventh publication in the Country Survey Series of the Human Relations Area Files Press, which comprises volumes on Afghanistan, British Borneo, Egypt, Iran, Jordan and the Russian Soviet Federated Socialist Republic, the book under review provides an interpretative description of present-day Cambodia. After a brief outline of the social and historical background the authors survey country and people, ethnic groups and languages, structure of government, political fractions, domestic affairs and foreign relations, economy, social organization, family life, education, cultural activities, religion, and finally customs and patterns of living. The text is supplemented

by a number of maps and tables. Though mainly intended for the layman, the book contains data and original observations which should prove of value to the student of theoretical sociology as well as to the economist and politician, interested in this country and in South-East Asian affairs in general.

Cambodia faces in many ways the same problems as other countries in these parts of the world, which attained independence a short while ago. Like the Federation of Malaya, for instance, it is underdeveloped and threatened by Communist expansion. The Cambodians, or Khmers, like the Malays, live on the land, while industry and trade are mainly in the hands of a Chinese minority, concentrated in the cities. Of the 275,000 Chinese in Cambodia 130,000 live in Phnom Penh. Since independence the Cambodian government, like the government of Tunku Abdul Rahman, has encouraged native businessmen to take the place of the Chinese. Cambodians are also frequently reminded by their leaders of their great historical past, particularly the Angkor Period (A.D. 802-1432). Yet, is it possible to influence the national character of a people? According to Steinberg the passive Cambodian outlook on life is intimately connected with their national religion, Theravada (or Hinayana) Buddhism. However, the mentality of Cambodians and Malays is very much alike, though the latter are Moslems. The book is a valuable contribution to the study and understanding of a small but important country in the heart of South-East Asia, whose present is overshadowed by a glorious past.

PRINCE JOHN LOEWENSTEIN

**Costumes of Upper Burma and the Shan States.** By R. A. Innes. Halifax (Halifax Museums), 1957. Pp. 58, 5 plates, 38 text figs.

**262**

This volume, begun as a guide to the Halifax Museum's collection of costumes from Upper Burma and the Shan States, has expanded in its progress into an account of the textile industry of that area. The costume of Shan, Kachin, Palaung, La'hu, Yang-lam, and Taung-yo women is described in detail and compared with particular reference to its material, embroidery, cut, tailoring and ornament. Shoulder bags, used by both sexes, are similarly described. There are also sections on textile fibres, dyeing, weaving, on the fabrics used, and on embroidery. There is a glossary, not of native words but of the terms used in English by weavers, and there is an index; there are 38 text figures, and there are five plates, one in colour, of the costumes as worn by Dr. Radha Sitaram of Rangoon.

The matter descriptive of the dresses has obviously been done in great detail with meticulous care, and, as far as the material available for study is concerned, is probably exhaustive. The other sections are necessarily limited to inference from the museum collections, and could probably be much expanded in the field. Thus it is probably a mere chance that jute is found used only in Kachin bags; the Angami Nagas frequently grow and use the fibre of *Pouzolzia viminea*, Wedd., for weaving, but it would be quite possible to form a casual collection of Angami costumes that happened not to include a garment woven either with this fibre or with that obtained from the cuticle of one of the nettles often used where durability rather than comfort or appearance is desired. It is possible that the 'non-spun filament' referred to in this section (p. 41) is actually a nettle fibre. So too in the section on dyeing it is rather strange that no form of madder is mentioned, a dye plant used from the Himalayas to Java, and one wonders whether either of the unidentified *mai hsalak* and *mak hpauny* may not be some species of *Rubia*. Apropos of the fixing of indigo by burying the yarn in wet clay one is forcibly reminded of the Angami Naga method of obtaining a real black. Indigo dye is obtained in the Assam hills generally not from an *indigofera* but from a *Strobilanthes*, and apparently needs no mordant, but when a true black is required by the Angami they boil the yarn with *Macaranga denticulata* and then steep it in mud, the iron salts in which act on the gallic acid in the *Macaranga* to give a deep, fast black.

The illustrations are good and the author is to be congratulated on the amount of careful work that he has put into a record of the costumes of tribes whose traditional customs and habits are probably by now in the melting pot.

J. H. HUTTON



**Les Musulmans Soviétiques.** By Vincent Monteil. Paris (Editions du Seuil), 1957. Pp. 190

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The problem of the cultural relations of peoples within the U.S.S.R. or China is not a simple one. While Monteil passes a number of aspects of life of the Islamic peoples of the Soviet Union under review, his categories of judgment are political; by this means he seeks for an answer to a politico-moral question. The prospects of the Islamic peoples are measured in terms of their national survival.

This is a work of high popularization. It is well informed on many questions of the life of the Moslems: colonialism, Russification, and Sovietization; the economic transformation of non-industrial societies in the U.S.S.R.; the position of women in Islamic society; the place of Islam in the ideology of Marxism; the history of the Islamic peoples during the Soviet period. Monteil is aware of the difference between the degree and type of Islamization of the nomads and erstwhile nomads, Kazakhs and Kirgiz, versus the Islamic life of the more sedentary Tadjiks and Azerbaidjanis. The scope of his work forbids the deeper penetration into any one topic at the expense of another.

The author is blessed with a good prose style and an ability to perceive and organize complex matters clearly. He comes to the object of his work with a background of many years of study and travel in the Islamic lands.

The unity of object within the book is assumed, but is not demonstrated. On the contrary, it can be readily shown that the modes of adherence to the banner of Islam among the Moslem peoples of the Caucasus, of Central Asia, of the Volga and Ural regions differ, and that their political and cultural unity is non-existent. Whatever over-arching organization exists among them has been imposed from without.

In his essay at political characterization, Monteil has not evolved an internally consistent position. The problem of colonialism is a troublesome one to him. On the one hand, he poses a desperate choice between liquidation of the Moslem peoples as a national entity, and liberation from the U.S.S.R. (pp. 186f.). On the other hand, he weighs in the political balance the status of the Islamic peoples; the result is favourable to the U.S.S.R. in terms of the degree of parliamentary representation and parliamentary democracy which the Moslem minorities enjoy (pp. 62-64).

An uneven work is this, one which reflects a failure to reach a final assessment of cultural and political relations among the different peoples of the U.S.S.R. In the quest for a political characterization, many other themes of vast moment in the development of the Caucasus and Central Asia are set aside. Among the chief of these are two concerning which a good literature exists, and the basis for judgment: one, the question of the concentration of industrialization and urbanization in enclaves, that is, the differential impact of Soviet life on the Moslems and other minority peoples within the territory of the U.S.S.R.; and two, the question of those cultural patterns and social institutions which aid and those which impede Sovietization. Will not these lines of investigation repay to a higher degree the effort of critique than a disquisition on old and new colonialism in the absence of a conception of colonialism?

LAWRENCE KRADER

**The Holy Land: New Light on the Prehistory and Early History of Israel.** *Antiquity and Survival*, Vol. II, No. 2-3. The Hague and Jerusalem, 1957. Pp. 77-317

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This book includes most valuable summaries of recent archaeological achievements in Israel, notably the excavations at Hazor and Nahariyah, the discovery of the Natufian burials at Eynan, the further opening of the Beth She'arim Necropolis, and explorations in the Negev and the Sinai Peninsula. It is also carefully designed to give a conspectus of the whole of Palestinian history from the Mesolithic to the Byzantine Period, paying special attention to what still remains to be discovered; it will serve as an excellent introduction to the whole subject. At the same time, by virtue of its generous illustrations, its thorough chronological table, and its special chapters on pottery, glass and coins, it will be a most useful reference volume for all Near Eastern archaeologists.

W. C. BRICE

**Adventures in the Nearest East.** By Cyrus H. Gordon. London (Phanix House), 1957. Pp. 192, 25 plates, 3 maps. Price £1 5s.

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The title is misleading, for though the first third of the book includes lively stories of the author's experiences with Glueck in Transjordan, and with expeditions at Gawra, Beit Mirsim and Ur, the main part consists of a series of accounts of ideas and customs at various times and places in the ancient Near East, as construed from seal engravings and writing. Thirty-two Mesopotamian seals are pictured and described, and there are summaries, with quotations, of the mythological texts of Ugarit, the legal records of Nuzu, the Lachish ostraca, the Dead Sea Scrolls, the anecdotes of the Coptic Fathers, and the Sassanian inscribed magical bowls.

The general thesis of the book is that very many of our roots can be traced back to the ancient Near East, and that an appreciation of this common heritage may be a valuable bond between peoples. Dr. Gordon gives several examples of our debt to the past, and his descriptions of former cultures are alive and vivid; but his outlook towards them is rather too severely modern to be really sympathetic. We are firmly reminded of the basic difference between science and magic or superstition (p. 160), and told that the necessity for a reappraisal of the New Testament, in the light of the Dead Sea Scrolls, is a foregone conclusion (p. 138).

It might have been better if views different from the author's had sometimes been acknowledged; not everyone would agree, for example, that the writings of the Qumranites are pre-Christian (p. 137). But in general Dr. Gordon presents in a novel and successful way a fair and stimulating account of the rewards and significance of archaeology in the Levant. The book is carefully produced, but the lower photograph opposite p. 49 was not taken at the site of Haran.

W. C. BRICE

**Jordan, Lebanon, and Syria: An Annotated Bibliography.** By Raphael Patai. New Haven (H.R.A.F. Press), 1957. Pp. vii, 289. Price \$6.50

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This book is one of the Human Relations Area Files Behavior Science Bibliographies. It contains 1,605 titles and is 'intended primarily for those interested in the peoples and cultures of contemporary Jordan, Lebanon, and Syria' (p. v). The bibliography is divided into four parts. Part I lists 780 titles containing material on two or more of the countries or on the Middle East in general. The other three parts treat Jordan (283 titles), Lebanon (222 titles), and Syria (320 titles) respectively. Each part is divided into a section on reference books, such as atlases, bibliographies, guide books, journals, etc., and a general bibliography. Items are listed alphabetically by author, and most are followed by brief and sometimes evaluative comments on the contents.

Part I is the least satisfactory. It is more loosely defined than the other sections, and selection of titles for inclusion has been more arbitrary. Why, for example, should Arberry's *Modern Arabic Poetry* be included (since we are told in the introduction that literature and arts must take second place) and not the same author's *Islam Today* which includes articles on both Syria and Jordan? One of Patai's own articles is listed both in Part I, as number 577, and under Jordan as number 977. These, however, are very minor faults in a work which must be of the greatest use to anyone contemplating research in the area.

The bibliography is expensive considering that it is paper-bound and badly reproduced by a photographic method.

BURTON BENEDICT

**Pioneer Peasant Colonization in Ceylon.** By B. H. Farmer. London (R. Inst. Internat. Aff. and O.U.P.), 1957. Pp. xxvii, 387, plates, tables, maps. Price £2 15s.

267

This book is peripheral to the interests of most social anthropologists, but will be of great interest to those familiar with Ceylon, or those who are concerned with the questions posed by the establishment of new settlements, called in Ceylon 'colony schemes.' The problems of soil, irrigation, health, agriculture and the general ecology of such schemes are discussed in detail. Also the



sociological problems raised by the colonies are considered at length and the many issues which face the administration fairly presented.

The superiority of Mr. Farmer's book over the earlier works on Asian colony schemes is marked both by its increased comprehensiveness and the detail with which the issues are treated, as well as by the way in which the interrelation between the manifold problems is stressed. More questions are posed than are solved, but this is only to be expected. The attempt by man to invent systematically an ecology for himself, rather than being a part of a naturally evolved system, is a new development and inevitably produces many scientific problems of great complexity which cross-cut the boundaries between traditional specialities. In such a situation a clear definition of the issues which need solution is invaluable, and frequently far more difficult than finding the answers.

Perhaps the most significant pointer for the future is Mr. Farmer's emphasis on the necessity for experiment—not only in experimental farms, laboratories, etc., but also in administration and in the organization of colonies. This variety of operational-sociological research is noticeably lacking in the thinking of most government bureaucracies, nor are they well suited to conduct such experiments and evaluate their own performance. The answer in Ceylon may lie in the adoption of a system similar to the evaluation branch established in the Indian planning machinery.

Mr. Farmer's book will, one hopes, be read by all who are concerned in any way with rural development in Asia. It has many lessons of more general application than to Ceylon's colony schemes.

M. Y. BANKS

**The Dynamics of a Rural Society.** By Ramkrishna Mukherjee. Berlin (Akademie-Verlag), 1957. Pp. x, 134, tables. Price DM. 12.70

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Ramkrishna Mukherjee is familiar to Africanists for his work in association with F. K. Girling in Uganda. In the present work he turns his attention once more to Bengal. The author offers the hypothesis that 'the function of the economic structure is of vital importance to their (peasant societies) course of development and without such an analysis it is not possible to appreciate the dynamics of these societies.' The book is therefore strongly recommended to those for whom this is not a self-evident proposition.

The argument is briefly that prior to British intervention the stable village community was inhabited by (for the most part) self-sufficient peasants. This pleasing picture reconstructed from fragmentary and scattered evidence was changed by the emergence under the British of higher and lower economic classes. These were the landed gentry on one side and sharecroppers and agricultural labourers on the other.

Although these changes in India were brought about by capitalist pressure upon British economic policy, the Indian three-class structure is not capitalistic but is characterized as semi-feudal. While capitalistic expropriation of peasant land and the establishment of commodity-production and wage labour might have developed in a free society, they were prevented in India by British colonialism.

There is, I think, evidence from some parts of India that whatever the ancient economic system the three-class structure had not such a dramatic emergence as Dr. Mukherjee suggests. We should have to know more than we do at present about the position of the kings and of traditional village-city relations before we could be sure. Dr. Mukherjee is not alone in thinking of the Hindu city as, in a sense, marginal to analysis.

This promising and stimulating book is marred by a tedious and lengthy conclusion. The author shows that the economic classes correspond fairly closely to major blocs of castes. I had hoped that this demonstration would be followed by a classical Marxian reduction of the caste system from the point of view of each class. This would be far more valuable than the worthless generalizations which we in fact get. The caste system irritatingly survives, we gather, because from being the old economic structure it has turned itself into a social structure and now 'dovetails' with the new economic structure. An example of this 'dovetailing' is the way in which

economically opposed groups are tricked into alliances based upon caste and community. Dr. Mukherjee's spleen one suspects is due more to peeve with his problem than to a liberal indignation. As usual, one is tempted to say, Marx's theories do not receive the respect they deserve in the hands of Marxists.

With all its emotion and misprints the book is recommended for its description of British economic policy in the nineteenth century, for the critique of Hindu peasant economy as egalitarian and for the description of the concomitance of caste blocs and economic class.

D. F. POCOCK

**India's Changing Villages.** By S. C. Dube. London (Routledge & Kegan Paul), 1958. Pp. xii, 230, 8 plates, map. Price £1 5s.

269

India's community development plans launched under the first five-year plan do not form a topic upon which a foreigner could prudently have an opinion. Since the opening of the second five-year plan doubts about their value have increased in India and some national newspapers have dismissed them as a flop. The Indian government however, stressing the delicacy, difficulty and originality of the task, rightly refuses to confuse the lack of universal success with total failure. In this book Dr. Dube attempts an appraisal of the villagers' reaction to the community projects by an account of two villages considered from this point of view. Although this is not the book one might have expected from the author of *Indian Village* it is a sober and objective piece of work. Dr. Dube neither praises nor blames. The anthropologist will however have to read between the lines in his attempt to understand the success or failure of any particular part of the scheme in these two villages.

Two interconnected points seem to emerge which are of general interest in our approach to new societies. The first is the urban orientation (at least at present) of the trained workers in the village and their tendency to take overmuch for granted the superiority of their techniques and their ethics. The second is the liberal glamour thrown over the whole project by the less hard-headed of its supporters. There is a disinclination to see that the situation is one of conflict between modern and traditional views. Such situations call for tactics and strategy rather than for 'good will.'

D. F. POCOCK

**Marriage and Family in India.** By K. M. Kapadia. 2nd ed. Bombay (O.U.P.), 1958. Pp. xxxii, 318. Price £1 7s. 6d.

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This book is a second edition, with minor modifications, of a work already reviewed in these columns (MAN, 1957, 11). A short section on the Kotas (pp. 93-96) has been added to the chapter on polyandry. And new material has been added to the discussion of the endogamous unit (pp. 118f.) and of bigamy and divorce (pp. 180f.), to show the results of recent surveys conducted by Dr. Kapadia and others on these matters. The greatest change occurs in the last chapter, where the final 20 pages are almost entirely new; they contain a fuller appraisal of the significance of the joint family, and contemporary data on the Nayar *tarwad* (based mainly on K. R. Unni's research). The lacuna in the index, noted in the previous review, remains uncorrected.

ADRIAN C. MAYER

**A Barbarian in India.** By Ralf Oppenheim. London (Phoenix House), 1957. Pp. 160, plates. Price £1 1s.

271

The author, a well-known Danish writer, here gives an account of an apparently not very long tour in India. What he found there consisted chiefly of corruption, snobbery, poverty and superstition, the last including a *sati* which he witnessed in Rajputana and which seems to have been performed quite openly. The strangers whom he meets show a surprising readiness to take him into their confidence and his memory for *verbatim* conversations is prodigious. What he tells us is in general true, but his wish to write vividly leads him to select the sensationally unpleasant. The translation is well done.

RAGLAN



## OCEANIA

**The Journals of Captain James Cook: I, The Voyage of the Endeavour, 1768-1771.** Edited by J. C. Beaglehole. Pp. cclxxxiv, 684, 45 illustrations and maps. Price £4. **Charts and Views.** Edited by R. A. Skelton. C.U.P. (for Hakluyt Soc.), 1955.

It is surprising, in view of their importance as source material and their literary merit, that we have had to wait until now for an adequate edition of Cook's journals. The volume and portfolio which have appeared so far (three more volumes are to follow) show that this edition will more than make good the deficiency. The portfolio of charts and views covers all three voyages; it does not include ethnographical material though it is naturally relevant to ethnographical studies. It is edited by Mr. Skelton, of the Map Room at the British Museum, whose help with the whole project is handsomely acknowledged by Mr. Beaglehole. The journal itself occupies about half of the present volume. The remainder is taken up with introductory matter and supplementary texts such as letters and official documents, contemporary press reports, extracts from journals kept by others on board the *Endeavour*, a calendar of documents and a list of the ship's company. There is full discussion of the sources and of previous editions of the journals, a 'Note on Polynesian History' which explains the confusing political situation in Tahiti (we are promised further ethnological matter in a later volume), and a General Introduction of a hundred pages which provides probably the best survey yet written of exploration in the Pacific before Cook's voyages. In fact the introductory matter would form a fair-sized and very valuable book in itself. The footnotes are informative, exhaustive when necessary and occasionally but never obtrusively humorous. The illustrations include maps and charts, portraits, and drawings, made during the voyage, of natural history, nautical and ethnographical subjects. Mr. Beaglehole's interests embrace not only bibliography and oceanic geography and ethnography, but also life in eighteenth-century England. This new edition is wholly admirable, and there could have been no better choice of editor for it.

B. A. L. CRANSTONE

**The Trumpet Shall Sound: A Study of 'Cargo' Cults in Melanesia.** By Peter Worsley. London (MacGibbon & Kee), 1957. Pp. iii, 290. Price £1 5s.

In the major part of this work Dr. Worsley has assembled and interpreted a wide range of data bearing upon millenarian cults of Melanesia. This in itself has been a valuable task, for which all students of the phenomena should be grateful, particularly since it has been carried out with thoroughness, accuracy and perspicuity. Many of the most informative statements about cargo cult are buried in out of the way reports written by government officials and missionaries in a variety of languages. These Dr. Worsley has tracked down and used effectively to round out the observations of anthropologists, who themselves have never been able to render a 'complete' account of any one cult. The area covered is from Netherlands New Guinea to Fiji; the time surveyed from 1857 to the present; and the type of institution mentioned varies from 'classical' cults such as Vailala Madness to more politically oriented movements such as those of Paliau and Tommy Kabu.

With this section, my reservations are minor. Dr. Worsley is already presenting his material under the influence of a definite theory; elements of his theory are stated before the reader has been prepared for them, either by data or by argument, and they lose in effectiveness. An unexceptionable premise is that cults are one possible reaction to an unbearable situation of political, social and economic domination. This leads the author to stress the exploitative, brutal and one-sided nature of the colonial relationship. The picture is true, but the focusing is partial.

There are several possible approaches to the explanation of cargo cult as a generalized phenomenon. There is the comparative study in the manner of Durkheim, in which the author would present detailed evidence, derived from critical cases, for each stage of a carefully marshalled argument. Dr. Worsley has been influenced towards such an approach, but the data of Melanesian cargo cults in

themselves are too uneven to make emulation possible. He has been driven instead to seek controls from outside his immediate subject. One method (rightly advocated, for example, by Judy Inglis in her article 'Cargo Cults, the Problem of Explanation,' *Oceania*, 1957) would be to draw upon analyses of cultural responses in other parts of Melanesia where cargo cults have not occurred. Dr. Worsley could have given a little more attention to this possibility; but he would not have been able to go very far. Anthropologists are drawn towards the exotic, and accounts of more orthodox political, religious and economic responses have not been published with sufficient frequency or detail. Instead, Dr. Worsley has resorted to comparison with millenarian cults in other cultures. Despite his wide-ranging references, I have the feeling that they have not helped his argument, and that they detract from the flow of the presentation.

There has been more than a little argument about the correct way to explain cargo cults. A great deal depends upon the level of abstraction; is the model to be one which considers only those elements common to all cults, or are there to be several models to be explained differentially? Both approaches should be followed up. Other references depend upon the universe of discourse—psychological or non-psychological, for example. Dr. Worsley's theory suggests that cargo cults in general are a response to situations in which there is acute want deprivation; they are political outlets of despair; the religious content functions as sanction and point of reference, uniting disparate or sectional elements; their form is moulded by the materialism of Melanesian religion, and its response to missions; they provide a selection from among a number of possible goals and ideas, and a symbolism which looks forward to a new morality.

Dr. Worsley rejects an approach based exclusively upon personality characteristics or continuation of indigenous forms because he rightly feels that this is crudely exploited in terms of European prejudice. The positive elements in his argument carry conviction, at least with me. But I think that he has lost something by being too abrupt with his negative arguments. He points out that Haddon said as long ago as 1917 that the permanence of native institutions is a false notion. But I feel that he does not give sufficient weight to the experimental nature of Melanesian religion, and to cargo cults as a continuation of a traditional type of religious experiment with obvious modification. The Taro and Baigona cults are cases in point. Similarly, the social history of the leadership, and the analysis of hysteria, while not perhaps necessary to the models of cargo cults in general, are highly significant for the consideration of differences.

This is a first-class review of a major topic of Melanesian anthropology; it enables us to put past work behind us and think clearly of new departure points for research.

CYRIL S. BELSHAW

**The Melanesians: Studies in their Anthropology and Folklore.** By R. H. Codrington. New Haven (H.R.A.F. Press), 1957. Pp. vii, 419, 33 illus. Price \$3.95.

This is an exact reproduction of Codrington's classic, complete with 1891 title page. Some of the illustrations have suffered a little, which was probably unavoidable, but the great value of having such standard works easily available once more at a moderate price needs no emphasis.

B. A. L. CRANSTONE

**We, the Tikopia.** By Raymond Firth. Second impression. London (Allen & Unwin), 1957. Pp. xxvi, 605, 15 plates, maps, plans, tables, genealogies. Price £2 10s.

This book, reviewed at length by Professor Forde in MAN, 1938, 25, is a classic that has long been out of print. This reissue in replica (though with only 15 of the 25 plates) is therefore exceedingly welcome. In the Introduction to the second printing (pp. xxvi), Professor Firth notes a few modifications that he would make if he were to rewrite the book, although he is quite happy to let it stand as it is. He also gives a list of papers on Tikopia published since 1936, or projected as a result of fieldwork by himself or Mr. J. Spillius, whether in the nineteen-thirties or in 1952-53.

M. A. BENNET-CLARK



# MAN

## *A Monthly Record of Anthropological Science*



**Remarkable New Finds at Ife, Western Nigeria**  
(Plate A)

**The Early Metallurgy of Copper and Bronze**  
A Report to the Ancient Mining and Metallurgy Committee of the Royal Anthropological Institute  
(with Plate B, four text figures and three tables)  
*Professor F. C. Thompson*

**Cross-Cousin Marriage among the Garo of Assam**  
(with four text figures)  
*Miss Chie Nakane*

**Obituary**  
Herbert Vander Vord Noone : 1880-1955  
*Dr. K. P. Oakley, F.B.A.*

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6 February	Significance of the Discovery of Tools with <i>Australopithecus</i> . DR. K. P. OAKLEY, F.B.A., F.S.A., F.G.S.
13 February	Excavations at Las Cuevas, British Honduras. ADRIAN DIGBY



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**Bronze Age Technology in Western Asia and Northern Europe: Part I**

(with Plate C and two text figures)

*Leon Underwood*

**Scimitars, Sabres and Falchions**

(with three text figures)

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20 February Some Thoughts on Chinese Bronzes. PROFESSOR S. HOWARD HANSFORD  
6 March The Bladesmith and His Forge in Assam. C. R. STONOR  
18 March Christianity and the Tswana. PROFESSOR I. SCHAPIRA. (Henry Myers Lecture, to be given in the rooms of the Royal Society, Burlington House.)



# MAN

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The 'Eskimo "Ulu"' in the Malayan Neolithic  
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*Prince John Loewenstein*

Bronze Age Technology in Western Asia and Northern Europe : Part II  
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27 March The Long-House in North-Western Europe. DR. I. C. PEATE, F.S.A.  
17 April (Title to be announced.) PROFESSOR A. E. JENSEN



# MAN

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The Hæmoglobins of 211 Cattle in Uganda

(with Plate F, a text figure and three tables)

*Dr. Hermann Lehmann and Dr. D. H. L. Rollinson*

A Kebaran Rock Shelter in Wadi Madamagh, near Petra, Jordan

(with Plate E and two text figures)

*Miss Diana Kirkbride*

Bronze Age Technology in Western Asia and Northern Europe : Part III

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24 April	Ritual in a Plural Society (Mauritius). DR. BURTON BENEDICT
1 May	Kinship and Community among the Southern Welsh. JOSEPH LOUDON, M.D.

### THE SELIGMAN IVORY MASK FROM BENIN: AN ANNOUNCEMENT TO ALL SUBSCRIBERS

Most subscribers to MAN will by now have heard that Mrs. B. Z. Seligman's famous ivory mask from Benin (recently reproduced on the R.A.I. Christmas Card, as well as in MAN for August, 1957) has been acquired by the Museum of Primitive Art, New York, for a sum which guarantees to it the highest possible status among ethnographical works of art. As a result of the sale, Mrs. Seligman, a Vice-President of the Royal Anthropological Institute, has with extraordinary generosity undertaken to contribute the proceeds of £20,000 to the Institute's recently opened Endowment Fund, provided that an equivalent sum can be raised from other sources towards the target of £50,000. More than £7,000 has in fact already been raised or promised, and a corresponding sum will be transferred to the Institute by the Trustees appointed by Mrs. Seligman. A special appeal is being made to Fellows and others.

It was thought that many Fellows and subscribers to MAN might wish to possess a fine reproduction of the mask because of its intrinsic beauty and importance, and before it left England it was specially photographed by Geoffrey Moss, A.R.P.S. A limited number of copies of this exceptionally beautiful photograph has been printed for sale by the Institute at 5 shillings, in aid of the Endowment Fund; they are very suitable for framing. Orders should be addressed at once to the Hon. Secretary at the Institute.



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Congenital Absence of the Basi-Occipital in a Romano-Briton  
(with Plate G and a table)

*Dr. D. R. Brothwell*

Mae Enga Time-Reckoning and Calendar, New Guinea  
(with a text figure and a table)

*M. J. Meggitt, M.A.*

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15 May	Distribution of Abnormal Hemoglobins in Algeria, Hoggar and High Volta. DR. RAYMOND CABANNES
22 May	The Cult of Yakan among the Lughara. DR. J. F. MIDDLETON
5 June	Early Man in Western North America. PROFESSOR L. S. CRESSMAN



# MAN

*A Monthly Record of Anthropological Science*



The Gypsy Bender Tent and its Derivatives  
(with Plate H)  
*James Walton*

A Note on Central African Dream Concepts  
(with a text figure)  
*J. H. Chaplin*

Shorter Notes  
A Symposium on Human Biology : Stone Implements from the Rub'al Khali

Correspondence

Reviews  
Asia : Europe : Oceania



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Hon. Assistant Editor of MAN: Miss M. A. Bennet-Clark, B.A.

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- 12 June Yoruba Urbanism. PROFESSOR WILLIAM BASCOM  
19 June Recent Stone Age Discoveries in Southern Africa. DR. J. DESMOND CLARK  
26 June Annual General Meeting, preceded by a garden party in Bedford Square



# MAN

## *A Monthly Record of Anthropological Science*



On a Benin Bronze Plaque Representing a Girl

(with Plate I)

*William Fagg*

Some Recent Archæological Work on the Tanganyika Coast

(with a map)

*Dr. G. S. P. Freeman-Grenville*

Proceedings of the Royal Anthropological Institute

The Yakan Cult Among the Lugbara

Shorter Notes

Boulder-Chip Scrapers in the Eastern Arctic : Guy Fawkes Day at Fresh Creek, Andros Island, Bahamas

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## R.A.I. CHRISTMAS CARD, 1958

Notice is given that a Christmas Card will again be printed for the use of Fellows this year. The subject provisionally chosen is the finest of the terra-cotta heads excavated at Ife, Nigeria, in January this year; it is perhaps the most beautiful and most subtly naturalistic of all the Ife sculptures yet discovered. A further announcement will be made in the August issue of MAN.

## CONDITIONS OF MEMBERSHIP

Any person interested in the sciences of anthropology and archaeology may apply for membership either through a Fellow or directly to the Hon. Secretary at 21 Bedford Square, W.C.1 (tel. Museum 2980), who will gladly supply full details of the Institute's activities and of the rights and obligations of Fellows. Forms of proposal should, in general, be signed by a Fellow who has personal knowledge of the applicant: but when an applicant does not already know a Fellow, the Hon. Secretary may be able to assist by listing Fellows resident near the applicant.

Fellows receive the *Journal* free, and may subscribe to *Man* at twenty-four (instead of thirty) shillings a year; they may borrow up to ten books at a time from the Library (by post, if desired), for a period of a month, or longer by arrangement; and, among other facilities, they may bring guests to lecture meetings.

The annual subscription of three guineas becomes due on election (unless this takes place in November or December), and on the first of every January thereafter. There is an entrance fee of one guinea: payment of fifty guineas entitles a Fellow to membership for life. Associate Membership, including most of the advantages of Fellowship, is open to persons under 26 at a subscription of one guinea, without entrance fee.



# MAN

*A Monthly Record of Anthropological Science*



**A Guatemalan Sacred Bundle**

(with Plate J and two text figures)

*Dr. E. Michael Mendelson*

**A 'Pelvimeter' for Orientation and Measurements of the Innominate Bone**

(with three text figures)

*Dr. S. R. K. Chopra*

**Shorter Note**

**An Early Mound at Luisville, British Honduras**

**Correspondence**

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**General : Africa : Europe**



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## R.A.I. CHRISTMAS CARD, 1958

All profits from the sale of this year's Christmas card will be devoted to the Institute's Endowment Fund. Fellows who order it will thus be directly helping the Institute to take advantage of the magnificent benefaction which Mrs. B. Z. Seligman has made with the proceeds—£20,000—of the sale of her Benin ivory mask, the subject of last year's card; under the special trust which she has set up, every pound which the Council and Fellows of the Institute can raise for the Endowment Fund will be matched by another from the trust. But the profits which can be made from the card will be severely restricted unless orders are placed in good time; last year the orders placed by the specified date did not seem to justify a printing of more than 2,500; we took a chance and printed 3,500, but in the event found that 4,500 could have been sold, so that many were disappointed and the Institute lost more than £30 in potential sales.

The subject of this year's card is again drawn from the West African field, in honour of the discovery last winter of a magnificent group of works of art at Ife in Nigeria, as was announced in a special plate (A) in the January issue of MAN. The piece chosen is a terra-cotta head, excavated in January, 1958, which is perhaps the most beautiful of all the Ife antiquities so far known. The photograph which is to be used on the front of the card will be published as an extra plate in the next issue of MAN (which will also include a full report on another important group of Ife antiquities). It is expected that the price, as last year, will be the very low one of 9 shillings a dozen for the first three dozen ordered, and 8 shillings for each additional dozen, plus the cost of postage; in the case of orders despatched overseas by the printers (on which Purchase Tax is not chargeable) these prices will be further reduced to 8 and 7 shillings respectively.

There seems no reason why this card should not attain a sale of 5,000 to 6,000 in aid of the Endowment Fund. But if sufficient are to be printed, it is imperative that all orders be placed at the Institute by 26 September, when the printing order must be given. The cards will then be ready for despatch a week or so later.



# MAN

## *A Monthly Record of Anthropological Science*



A Terra-Cotta Head Excavated at Ife, 1958  
(Plate K)

The Ore Grove at Ife, Western Nigeria  
(with Plate L and four text figures)  
*K. C. Murray and Frank Willett*

The Occurrence of the Trait for Hæmoglobin *J* in a Chinese  
(with a table)  
*Dr. F. Vella, Dr. R. H. C. Wells and Dr. Leong Hon Koon*

Shorter Note  
Enthusiasm and Restraint in the Study of Human Evolution  
*Dr. E. H. Ashton*

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Hon. Editor of MAN: W. B. Fagg, M.A.

## R.A.I. CHRISTMAS CARD, 1958

The terra-cotta head, excavated at Ife, Nigeria, early this year, which is illustrated in Plate K facing this notice, is the subject chosen for the Institute's greetings card for Christmas, 1958. It is considered to be probably the most sensitively modelled of all the Ife sculptures.

Sales of the card will be in aid of the Institute's recently launched Endowment Fund, and all profits made on it will be doubled by a corresponding transfer to the Fund from the trust fund set up by Mrs. B. Z. Seligman. Fellows are therefore urged both to buy the cards liberally and to place their orders immediately, and in any case not later than 26 September, so that a sufficiently large printing can be undertaken to take full advantage of the eventual demand. Please use the form enclosed with this issue, or write by air letter from abroad.

Prices: 9 shillings a dozen for the first three dozen, 8 shillings for each additional dozen. A further discount of one shilling a dozen is given for orders despatched overseas by the printers (on which Purchase Tax is not chargeable). Postage is charged extra in all cases.

## NOTICE TO CONTRIBUTORS

The Hon. Editor of MAN gives notice that he will be absent on fieldwork in Nigeria from 9 October, 1958, until about 20 April, 1959, but has made arrangements to publish MAN normally during this period. Sufficient material has been edited, and will be made up into issues before he leaves, for the period up to and including May, 1959, with the exception of the correspondence columns, which will be kept up to date as usual. In order to assist him in maintaining regularity of publication, he trusts that—apart from letters for publication, which will be welcome—only the most pressing communications (addressed to W. B. Fagg, Hon. Editor of MAN, c/o The Museum, Ife, Western Nigeria) will be sent to him in the field. Others may be addressed to him at the British Museum, London, W.C.1, to await his return. He begs indulgence for any inconvenience which these arrangements may cause to contributors and others.



# MAN

## *A Monthly Record of Anthropological Science*



### Morhana Pahar : A Rediscovery

(with Plate M)

*Mrs. Bridget Allchin, Ph.D.*

### A Note on the Durability of Malay Marriages

*Michael G. Swift*

### Shorter Notes

A Gordon Childe Memorial : Tents and Domes in Persia : A Witch Post from Scarborough

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## R.A.I. CHRISTMAS CARD, 1958

Supplies will shortly be available of this year's card, of which the subject is the magnificent terra-cotta head from the ancient art of Ife in Western Nigeria, which was excavated there earlier this year and was illustrated in Plate K in the September issue of MAN. The block has been altered to improve the proportions of the background to the head; it will be printed on a cream-based card about 7 x 4½ inches in size.

Sales of the card are in aid of the Institute's recently launched Endowment Fund, and all profits made on it will be doubled by a corresponding transfer to the Fund from the trust fund set up by Mrs. B. Z. Seligman. Fellows are urged to buy the cards liberally and so to benefit the Fund while spreading knowledge of the Institute with their greetings to their friends. An order form was enclosed with the September issue.

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## NOTICE TO CONTRIBUTORS

The Hon. Editor of MAN gives notice that he will be absent on fieldwork in Nigeria from 9 October, 1958, until about 20 April, 1959, but has made arrangements to publish MAN normally during this period. Sufficient material has been edited, and will be made up into issues before he leaves, for the period up to and including May, 1959, with the exception of the correspondence columns, which will be kept up to date as usual. In order to assist him in maintaining regularity of publication, he trusts that—apart from letters for publication, which will be welcome—only the most pressing communications (addressed to W. B. Fagg, Hon. Editor of MAN, c/o The Museum, Ife, Western Nigeria) will be sent to him in the field. Others may be addressed to him at the British Museum, London, W.C.1, to await his return. He begs indulgence for any inconvenience which these arrangements may cause to contributors and others.



# MAN

## *A Monthly Record of Anthropological Science*



**A Negro Community in Yugoslavia**  
(with Plate N and three text figures)  
*Alexander Lopashich, Dr.Phil.*

**The Boskop 'Race' Problem**  
(with two tables)  
*Dr. Ronald Singer*

**Proceedings of the Royal Anthropological Institute**  
**The Group as the Unit of Social Evolution**  
*Dr. Margaret Mead*

**Shorter Note**  
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(with three text figures)  
*Dr. I. M. Lewis*

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### R.A.I. CHRISTMAS CARD, 1958

Supplies are now available at the Institute of this year's card, with its reproduction of the magnificent terra-cotta head excavated at Ife in Western Nigeria in February this year, and illustrated as Plate K in the September issue of MAN; with other important finds from the Ita Yemoo site (including the bronzes illustrated in Plate A in the January issue), it was recently on exhibition for four months in the British Museum, and is now being shown at the Museum of Primitive Art, New York, before returning to Nigeria by way of Paris.

The cards, supplied with envelopes, are about 7 x 4½ inches in size; the proportions of the block have been improved since its appearance as Plate K.

Sales of the card are in aid of the Institute's recently launched Endowment Fund, and all profits made on it will be doubled by a corresponding transfer to the Fund from the trust fund set up by Mrs. B. Z. Seligman. Fellows are urged to buy the cards liberally and so to benefit the Fund while spreading knowledge of the Institute with their greetings to their friends. An order form was enclosed with the September issue.

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## FORTHCOMING MEETINGS OF THE INSTITUTE, 1959

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 5 February Neighbourliness in Bunyoso. J. H. M. BEATTIE, B.LITT., M.A., D.PHIL.  
 5 March Mothers and Daughters: A Discussion of Kinship in Contemporary Britain. PETER WILLMOTT, Ass.  
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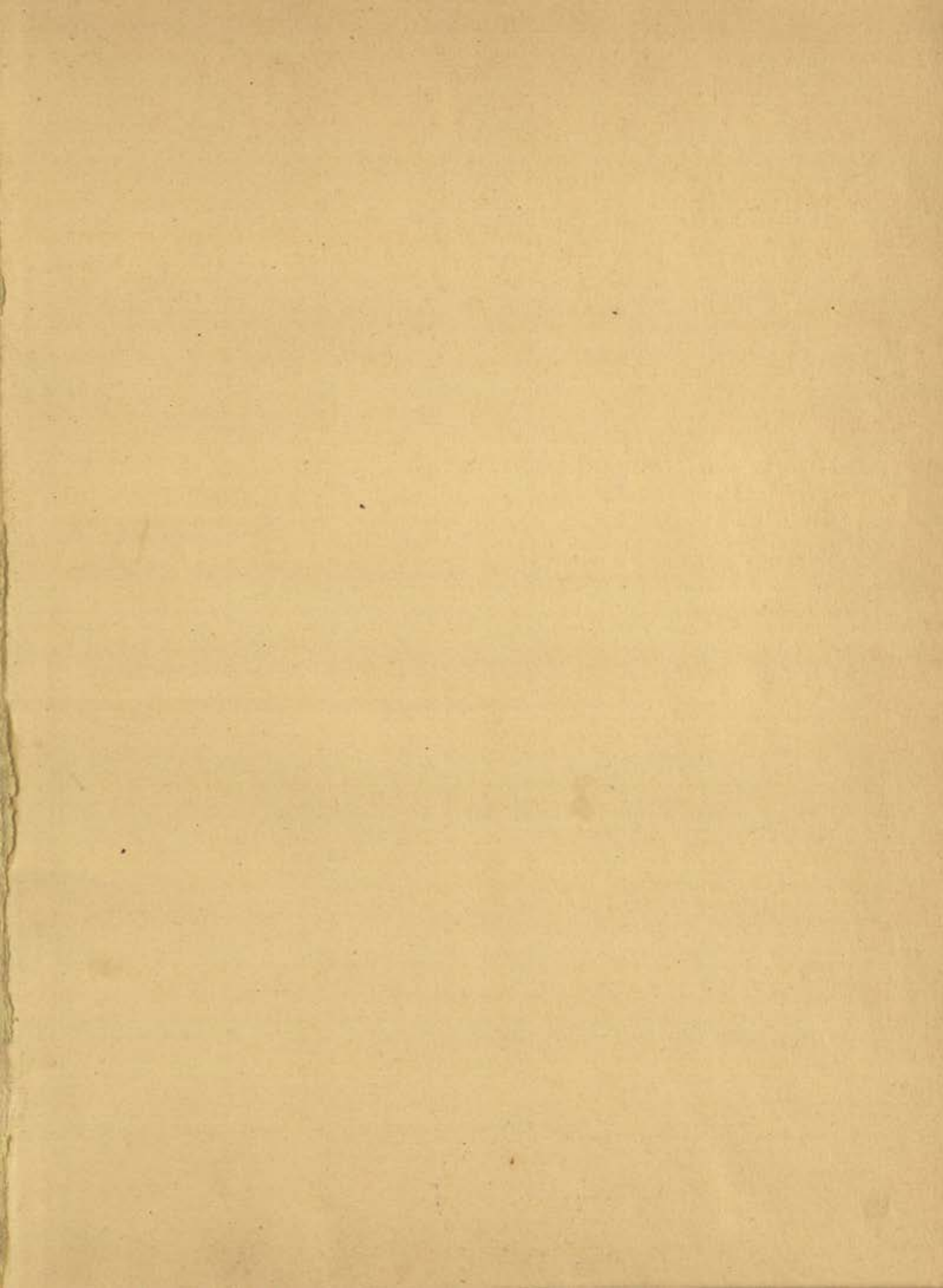
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